PROCEEDINGS

OF

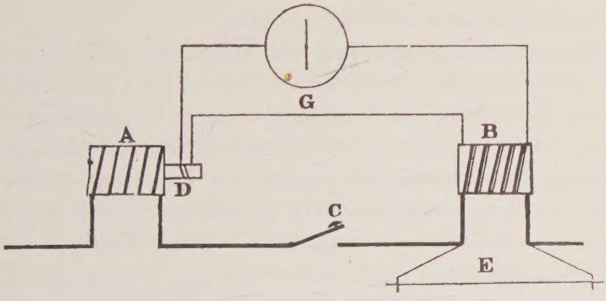
THE PHYSICAL SOCIETY

OF LONDON.

JULY 1887.

I. *Note on* Prof. Carey Foster's *Method of Measuring the Mutual Induction of two Coils. By* JAMES SWINBURNE *.

PROF. FOSTER suggests at the end of his paper † that his method may be of use in dynamo work. Wishing to get some arrangement for testing the induction through any part of a dynamo, and for testing samples of iron for Messrs. Crompton and Co., the writer devised an arrangement which dispenses with the objectionable ballistic galvanometer. It was tried roughly last summer, with the view of making a permanent arrangement if the method worked well.



The primary current was led through an electromagnet, A,

* Read February 26, 1887.

† Proc. Physical Society, vol. viii. p. 137.

which represented the dynamo, and through one wire of a double-wound coil, B, which represented a pair of coils of known mutual induction. This coil was shunted by a wire, E, with sliding contacts. This circuit was made or broken by the switch C. A pilot wire, D, round the model electromagnet was connected in series with the secondary coil of B and with the ordinary reflecting-galvanometer, G. The arrangement was fairly sensitive; but had one fault, which was in fact foreseen. Instead of no current, the galvanometer gets a current first one way and then the other, so that the spot jerks each way. This might be remedied by another arrangement of the wires; but a similar difficulty will arise in dynamo work. The flux of the induction through the standard coil varies as the flux of the primary current; but the flux of the induction through, say, a section of a field-magnet depends on the saturation of the iron. To avoid these difficulties, a galvanometer with a heavy ballistic astatic needle of the kind introduced by Ayrton and Perry will be used. This has been made by Mr. Dobson, lately one of Messrs. Crompton's pupils. It is of somewhat novel design. The coils, of which there are several sets, can be changed without dismounting the needle, so that the instrument can be used as a high or low resistance or differential galvanometer; and can be also used as a ballistic, or as a flywheel or integrating galvanometer. If necessary, little arms with balls like those of a minute fly-press will be added. The final apparatus was not put in hand till quite lately, as there was no suitable room available for such work; but it is now being made.

It was intended to calibrate the induction-coils from coils whose mutual induction could be calculated; or from a dynamo by measuring the induction through the armature with a known exciting current, and then running the dynamo and measuring its electromotive force and speed. This method has, by the way, been recently mentioned by M. Kapp, for calibrating a ballistic galvanometer. Prof. Foster's method seems infinitely more convenient, and may save much trouble if a good condenser is to be had. Of course an ammeter will be in the primary circuit.

It would seem that a flywheel-galvanometer might be used for such purposes as finding the ohm from a pair of coils with

calculated mutual induction. Suppose, for instance, a contact-breaker makes and breaks contact in the primary circuit with a known frequency, and allows the impulses one way in the secondary coil to go round one coil of a differentially-wound flywheel-galvanometer. A constant current in the other coil is regulated to oppose these impulses; and from these the ohm might be got. Either the contacts must be made slowly enough for the current in the primary to come to its permanent value, or a correction must be made for the error. There is also a small error due to capacity. This suggestion is made with hesitation, as it seems probable that somebody must have thought of the arrangement or tried it. It is infinitely easier to think of a novelty, or an apparent novelty, than to find out whether it is really new. For instance, on looking the matter up a little for this note, the writer found that the null method of comparing the coefficients of mutual induction of coils is mentioned in a footnote to Brillouin's elaborate memoir on the comparison of coefficients of induction. Maxwell also mentions a loaded differential galvanometer for comparing frequent condenser-discharges with a known current. Roïti's method is also given in Mascart and Joubert's book.

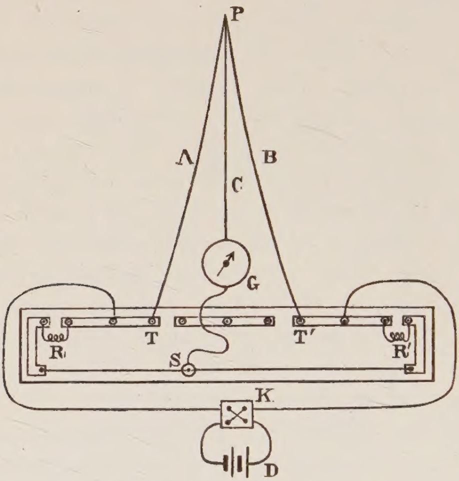
II. *On the Electrical Resistance of Vertically-suspended Wires.* *By* Shelford Bidwell, *M.A., F.R.S.**

From the experiments to be described in this paper, it appears probable that the electrical resistance of vertically-suspended copper and iron wires alters to a small extent with the direction of the current traversing them. If the wire is of copper, the resistance is slightly greater when the current goes upwards than when it goes downwards; while, on the other hand, the resistance of an iron wire is apparently greater for downward than for upward currents†.

* Read March 12, 1887.

† Venturing to imitate the fanciful analogy used by Sir William Thomson, who, in discussing the thermoelectric effect now universally associated with his name, speaks of the "specific heat" of electricity, we may perhaps also speak of the "specific gravity" of electricity, and say that (like its specific heat) it is positive in copper and negative in iron.

The arrangement employed for exhibiting this effect is shown in the annexed diagram. A wire, A B, of the material to be tested is suspended at its middle point, P, from a support 10·5 metres above a metre-bridge, to the terminals, T T′, of



which the ends of the wire are connected. Another wire, C, is soldered at one end to P, and connected through the galvanometer, G, with the slider, S. A resistance of 100 ohms is inserted in each of the gaps, R R′, and a commutator, K, is interposed between the two-cell battery, D, and the bridge.

With this arrangement, supposing that the two halves of the wire A B are of uniform sectional area and in the same physical condition, and that the various parts of the apparatus are in fair order and adjustment, there will be a balance when the slider is near the middle division of the scale. And if the resistances in the circuit are independent of the direction of the current, it is clear that the balance will be maintained notwithstanding that the commutator K be reversed. But this is found not to be the case.

A series of experiments was made with a copper wire ·4 millim. in diameter (No. 28 B.W.G.), and having a total resistance of 2·11 ohms. The commutator was first set so that the current through the wire passed up the portion B and down the portion A (*i. e.* in the direction B P A), and a balance was obtained by adjusting the slider. The commu-

tator was then reversed and the current made to pass up A and down B. This at once destroyed the balance, and in order to restore it, it was necessary to move the slider several divisions towards the right. Assuming that the total resistance of the wire remains constant, this result may be explained by supposing that the reversal of the current is accompanied by increased resistance in the portion A, and diminished resistance in the portion B. Owing to its vertical suspension, the resistance of that portion of the wire in which the current travels upwards is greater than it would be if the wire were placed in a horizontal position, while the resistance of the portion in which the current travels downwards is less.

The experiment was repeated with an iron wire of larger size, its diameter being ·8 millim. (No. 22 B.W.G.). With this the effect of reversal was smaller; but it was well marked, and of the opposite nature to that observed in the former case.

The readings obtained in the two series of experiments are given in the following Table :—

Number of experiment.	Scale-readings.		Difference.
	Current direct.	Current reversed.	
Copper Wire.			
1.	569	633	−64
2.	567	637	−70
3.	595	651	−56
		Mean difference	−63·3
Iron Wire.			
1.	780	770	+10
2.	760	748	+12
3.	759	748	+11
		Mean difference	+11

I believe these effects are associated with certain thermo-electric phenomena discovered by Sir William Thomson. In his famous Bakerian lecture, published in the Philosophical Transactions for 1856, he showed that if a stretched copper wire is connected with an unstretched wire of the same metal and the junction heated, a thermoelectric current will flow from the stretched to the unstretched wire through the hot junction; while, if the wires are of iron, the direction of the current will be from unstretched to stretched. It follows, therefore, from the laws of the Peltier effect, that if a battery-current is caused to flow from a stretched to an unstretched wire, heat will be absorbed at the junction when the metal is copper, and will be developed at the junction when the metal is iron: and if the direction of the current is reversed the thermal effects will also be reversed.

Now a vertically suspended wire is unequally stretched by its own weight, the stress gradually increasing from zero at the lowest point to a maximum at the highest. Any small element of the wire is more stretched than a similar element immediately below it, and less stretched than a neighbouring element just above it. Thus a current of electricity, in passing from the lowest to the highest point of such a wire, is always flowing from relatively unstretched to relatively stretched portions. If, then, the wire were of copper, heat would be evolved throughout its whole length; the temperature of the wire would rise, and its resistance would consequently be increased. With a current flowing from top to bottom, the temperature of the wire would fall and its resistance diminish. So also an iron wire would be cooled and have its resistance lowered by an upward current, while a downward current would heat it and increase its resistance. The changes of resistance are thus, as I believe, proximately due to changes of temperature.

The resistance of the bridge-wire used in my experiments was ·244 ohm, and, as already mentioned, an additional resistance of 100 ohms was placed in each of the gaps adjoining the bridge-wire. Denoting the resistance of the half A of the suspended wire by a, and that of B by b, we have, from the first experiment with the copper wire (the result of which agrees closely with the mean):—

For direct current,

$$\frac{a}{b}=\frac{100^{\omega}+\cdot569\times\cdot244^{\omega}}{100^{\omega}+\cdot431\times\cdot244^{\omega}}$$

$$=\frac{100139}{100105}.$$

Also

$$a+b=2\cdot11^{\omega}.$$

Hence

$$a=1\cdot0551792^{\omega}\ *,$$

$$b=1\cdot0548208^{\omega}.$$

For reversed current,

$$\frac{a}{b}=\frac{100^{\omega}+\cdot633\times\cdot244^{\omega}}{100^{\omega}+\cdot367\times\cdot244^{\omega}}$$

$$=\frac{100154}{100090}.$$

And, as before,

$$a+b=2\cdot11^{\omega}.$$

Hence

$$a=1\cdot0553372^{\omega}*,$$

$$b=1\cdot0546628^{\omega}.$$

When therefore the current was reversed, the value of a was increased by

$$1\cdot0553372-1\cdot0551792 \text{ ohm}$$

$$=\cdot000158 \text{ ohm}.$$

This is equivalent to about 16 thousandths per cent.

Assuming that a change of temperature of 1° C. produces an alteration of ·4 per cent. in the resistance, it follows that the temperature of the copper wire was $\frac{1}{25}$ degree C. higher with an upward than with a downward current.

The current traversing the wire was not measured, but it was probably about 1 ampere.

It will be seen from the figures in the Table that the

* Of course the resistances are not really measured to the high degree of accuracy suggested by these figures; but any small error of excess or defect would be approximately the same for the two values of a (with direct and reversed currents) and would not materially affect their difference, to which alone importance is attached.

changes which occurred in the resistance of the iron wire were considerably smaller than those observed in the case of copper. This was unexpected, since the thermoelectric effects are, I believe, somewhat greater with iron. But the apparent anomaly is obviously to be accounted for, at least in part, by the higher specific resistance of iron. With the same electromotive force the current per unit of sectional area would be six or seven times greater in copper than in iron, and the Peltier effect is proportional to the current. To render the results in the two cases strictly comparable, other less important differences, such as those of specific heat and radiating-power, would have to be taken into account.

If a convenient opportunity offered it would be satisfactory to repeat the experiments with much longer wires, such as might be suspended in the shaft of a coal-pit or in a shot-tower. The effects hitherto observed are so small that they might possibly be due to accidental causes, and I publish this account of them with some diffidence.

III. *On the Production, Properties, and some suggested Uses of the Finest Threads.* By C. V. Boys, *Demonstrator of Physics at the Science Schools, South Kensington* *.

I HAVE lately required for a variety of reasons to have fibres of glass or other material far finer than ordinary spun glass; I have therefore been compelled to devise means for producing with certainty the finest possible threads. As these methods may have some interest, and as some results already obtained are certainly of great importance, I have thought it desirable to bring this subject under the notice of the Physical Society, even though at the present time any account must of necessity be very incomplete.

The subject may be naturally divided, as in the title, into three parts.

1. *Production.*

The results of the natural methods of producing fibres by living things, as spiders, caterpillars, and some other creatures,

* Read March 26, 1887.

are well known ; but it is useless to attempt to improve on Nature in this direction by our own methods.

Fibres are also produced naturally in volcanoes by the rushing of steam or compressed gases past melted lava, which is carried off and drawn out into the well-known Pelés hair. The same process is employed in making wool from slag, for clothing boilers, &c. ; but in each of these cases the fibres are matted together, they are not adapted to the requirements of a Physical Laboratory. By drawing out glass softened by heat by a wheel we obtain the well-known spun glass.

There is a process by which threads may be made which is natural in that natural forces only are employed, and the thread is not in any way touched during its production. This is the old, but now apparently little-known experiment of electrical spinning. If a small dish be insulated and connected with an electrical machine and filled with melted rosin, beeswax, pitch, shellac, sealing-wax, Canada balsam, guttapercha, burnt india-rubber, collodion, or any other viscous material, the contents will, if they reach one edge of the dish, at once be shot out in the most extraordinary way in one, two, or it may be a dozen threads of extreme tenuity, travelling at a high speed along "lines of force." If the material is very hot, the liquid cylinders shot out are unstable and break into beads, which rattle like hail on a sheet of paper a few feet off. As the material cools, the beads each begin to carry long slender tails, and at last these tails unite the beads in twos and threes ; but the distance between the beads is far greater than that due to the natural breaking of a cylinder into spheres, as after the first deformation of the surface occurs which determines the ultimate spheres the repulsive force along the thread continues, and drags them apart many times their natural distance. As the temperature continues to fall and the material to become more viscid, the beads become less spherical, and the tails less slender, and at last a perfectly uniform cylindrical thread is formed. If sealing-wax is employed, and a sheet of paper laid for it to fall on, the paper becomes suffused in time with a delicate rosy shade produced by innumerable fibres separately almost invisible. On placing the fingers on the paper, the web adheres and can be raised in a sheet as delicate and intricate as any spider's-web.

It is interesting to see how these fibres fly to any conducting body placed in their path. If the hand is held there it is quickly surrounded by a halo of the finest threads. If a lighted candle is placed in the way, the fibres are seen by the light of the candle to be rushing with the greatest velocity towards it, but when a few inches off they are discharged by the flame, they stop, turn round, and rush back as fast into the saucer whence they came. The conditions for the success of this beautiful experiment are not very easily obtained*.

Fibres spun by the electrical method are so brittle that they do not seem to be of any practical use. It is possible, however, that this method might be available for reducing to a fine state of division such of the rosins or other easily fusible bodies as cannot readily be powdered mechanically.

On returning to bodies which, like glass, require a high temperature for their fusion, to which the electrical method is inapplicable, we find that the only method practically available is that of drawing mechanically. It would seem that if finer threads than can be formed by the ordinary process of glass-spinning were required, it would be necessary to obtain a higher speed, to have the glass hotter, and to have as small a quantity as possible hot. I put this idea to a test by mounting at the back of a blowpipe-table a pair of sticks which could be suddenly moved apart by a violent pull applied to each near their axes. By these means the upper ends were separated about 6 feet, and the motion was so rapid that it was impossible to follow it. A piece of glass drawn out fine was fastened to the end of each stick, and the ends of these heated by a minute blowpipe-flame. They were immediately made to touch and allowed to fly apart. In this way I obtained threads of glass about 6 feet long, finer than any spun glass I have examined. By using the oxyhydrogen jet with the same apparatus, still finer threads were produced. It was evident then that the method was right; but some more convenient device which also would make long threads would be preferable.

There are several ways of obtaining a high speed, the most usual depending on an explosive; but it would be difficult to arrange in a short time a gun which could be used to shoot

* If the wick of the candle is connected with the opposite pole of the machine, the threads at one stage are sure to return to the saucer.

a projectile carrying the thread which would not also destroy the thread by the flash. It is possible that an air-gun could be so arranged. Rockets when at the period of most rapid combustion have an acceleration which is enormous. Thus a well-made 2-oz. rocket is at one part of its flight subject to a force of over 3 lb. in gravitative measure. This force, acting on such a body for 10 seconds only, would, neglecting atmospheric resistance, starting from rest, carry it more than 6 miles. The acceleration is about 28 times that due to gravity on the earth, or about the same as that on the sun. Anyone who will stay in a room with a lighted two-ounce rocket, having no stick or head, will obtain a more vivid notion of the value of gravity on the sun than in any other way I know.

A rocket is perhaps more available for thread-drawing than a gun, but it does not seem altogether convenient. One other method, however, is so good in every respect, that there seems no occasion to try a better. The bow and arrow at once supply a ready means of instantly producing a very high velocity, which the arrow maintains over a considerable distance. For the special purpose under consideration, the lightest possible arrow is heavy enough. I have made arrows of pieces of straw, which may be obtained from wool-shops, a few inches long, having a needle fastened to one end for a point. Arrows made in this way travelled the length of the two rooms in which I made these experiments—about 90 feet—in what seemed to be under half a second. They completely pierced a sheet of card at that distance, which I put up thinking that a yielding target might damage them less than the wall, and were then firmly stuck unharmed in the wall behind; in every way they behaved so well that I do not think a better make of arrow possible.

The bow I used was a small cross-bow held in a vice with a trigger that could be pulled with the foot. The first bow was made of oak, the first wood that came to hand. I then made some bows of what was called lance-wood (it was unlike any lance-wood I have seen); but the trajectory was at once more curved, the arrow took perceptibly longer to travel, and the threads produced were thicker. As the arrow is so light, the only work practically that the bow has to do is to move itself; that wood then which has the highest

elasticity along the fibres for its mass is most suitable; in other words, that wood which has the greatest velocity of sound is best. I therefore made bows of pine, and obtained still higher velocities and finer threads than I could obtain with oak bows.

With a pine bow and an arrow of straw I have obtained a glass thread 90 feet long and $\frac{1}{10000}$ inch in diameter, so uniform that the diameter at one end was only one sixth more than that at the other. Pieces yards long seemed perfectly uniform.

A fragment of drawn-out glass was attached to the tail of the arrow by sealing-wax, and heated to the highest possible temperature in the middle, the end being held in the fingers. With every successful shot the thread was continuous from the piece held in the hand to the arrow 90 feet off. The manipulation is, however, difficult, but another plan equally successful has the advantage of being quite easy. It is not necessary to hold the tail of glass at all; if the end of the tail only be heated with the oxyhydrogen jet until a bead about the size of a pin's head is formed, and the arrow shot, this bead will remain behind on account of its inertia, and the arrow go on, and between them will be pulled out the thread of glass.

Prof. Judd has kindly given me a variety of minerals which I have treated in this manner. Some behave like glass and draw readily into threads, some will not draw until below a certain temperature, and others will not draw at all, being either perfectly fluid like water, or when a little cooler perfectly hard.

Among those that will not draw at all may be mentioned Sapphire, Ruby, Hornblende, Zircon, Rutile, Kyanite, and Fluorspar.

Emerald and Almandine will draw, but care is required to obtain the proper temperature. In the case of the Garnet Almandine, if the temperature is too high, the liquid cylinder, if formed, breaks up, and a series of spheres fall on the table in front of the bow. At a slightly lower temperature the thread is formed, but it is beaded at nearly regular intervals for part of its length.

Several minerals, especially complex silicates as Orthoclase,

draw very readily, but that which surpasses all that I have tried at present is Quartz, which, though troublesome in many ways at first, produces threads with certainty. It required far more force to draw quartz threads than had been previously experienced. The arrow, instead of continuing its flight, hardly disturbed by the drag of the thread, invariably fell very low, and was not in general able to travel the whole distance. So great is the force required that I split many arrows before I succeeded at all. I have obtained threads of quartz which are so fine that I believe them to be beyond the power of any possible microscope. Mr. Howes has lent me a $\frac{1}{15}$-in. Zeiss of excellent definition, and though, on looking at suitable objects, definite images appear to be formed on which are marks corresponding according to the eyepiece-micrometer to $\frac{1}{100000}$ inch, yet these threads are hopelessly beyond the power of the instrument to define at all. On taking one that tapers rapidly from a size which is easily visible, the image may be traced until it occupies a small fraction of one division, of which 13·4 correspond to $\frac{1}{1000}$ inch on the stage; then the diffraction-bands begin to overlap the image until it is impossible to say what is the edge of the image. Having reached this stage, the thread may be traced on and on round the most marvellous convolutions, the diffraction-fringe now alone appearing at all, but getting fainter and apparently narrower until the end is reached. That a real thing is being looked at is evident, for if the visible end is drawn away the convolutions of fringes travel away in the same direction. It is impossible to say what is the diameter of these threads; they seem to be certainly less than $\frac{1}{100000}$ inch for some distance from the end.

It might be possible to calculate what would be the appearance presented by a cylinder of given refractive power, and 1, 2, 3, &c. tenths of a wave-length of any kind of light in diameter, when seen with a particular microscope. By no other means does it seem possible to find out what the true size of the ends of these threads really is.

2. *Properties.*

I can at present say very little of the properties of these very fine fibres; I am now engaged with Mr. Gregory and Mr. Gilbert in investigating their elasticity. The strength

goes on increasing as they become finer, that is, when due allowance is made for their reduced sectional area, and it seems to reach that of steel, about 50 tons to the inch in ordinary language; but on this point I have not yet made any careful experiments.

The most obvious property of these fibres is the production of all the colours of the spider-line when seen in a brilliant light. The most magnificent effect of this sort I have seen, was produced by a thread of almandine. One of these the length of the room, even though illuminated with gas-light only, was glistening with every colour of the rainbow. In attempting, however, to wind it up, it vanished before me. It is of course only visible in certain directions.

The chief value of threads to the physicist lies in their torsion. Spun glass, as is now well known, cannot be used for instruments of precision, because its elastic fatigue is so great that, after deflection, it does not come back to the original position of rest, but acquires a new position which perpetually changes with every deflection. If left alone, this position slowly works back towards a definite place more rapidly as it is further from it.

To compare threads made of different materials, I made a flat cell in which a galvanometer-mirror, made by Elliot Bros., might hang, being attached to the lower end of the thread. The upper end was secured to a fixed support, and a fixed tube protected the length of the fibre from draught. The cell, which could be moved independently of the rest, was protected by a cover. By means of a lamp and scale, the exact position of rest of the mirror could be determined with great accuracy. On turning the cell round as many times as might be desired, the mirror was turned with it, and could be left any time in any position. On turning the cell back again, the mirror was allowed to come to its new position of rest, air-resistance of the cell bringing about this result in a few swings. By this means I hoped quickly and accurately to determine the fatigue in a variety of threads, but an unforeseen difficulty arose which I cannot yet explain. When the cell was moved round slightly so as not to touch the mirror, the mirror moved at first in the same direction as was to be expected, but it came to rest in a new position, to reach which it had to move in the opposite

direction to the movement of the cell. Whichever way the cell was shifted, the mirror always went the other to find its position of rest. Thinking that it or the cell were electrified, I damped both by breathing on them, but with no result, and the next day the same effect was observable. So great was this effect that I could set the cell with greater accuracy by watching the spot of light than by the pointer carried by the cell working over a 4-inch circle.

Thinking that magnetism might have something to do with this effect, I brought a horseshoe-magnet near the mirror, when it was instantly deflected through a large angle. An examination of the cement used (Loudon's bicycle cement) showed that it was magnetic. Of many cements examined, sealing-wax was more nearly neutral than any other. Bicycle cement and electrical cement were strongly magnetic; all others except sealing-wax strongly diamagnetic. The apparatus was therefore taken to pieces and carefully cleaned. It was put together with as small a quantity of sealing-wax as possible, and the mirror was attached to a fragment of thin pure copper wire, which again was fastened by a speck of sealing-wax to the thread. Even then the same kind of effect as that already described occurred. Still a magnet deflected the mirror, but not so much, and the cell was practically neutral; yet, when the cell was turned a little, the mirror changed its position of rest.

Without pursuing this question further, I put a window in the protecting tube and turned the mirror by means of a small instrument passed up from below. Thus neither window nor support were moved. A piece of spun glass nearly 9 inches long gave a period of oscillation to the mirror of 2·3 seconds about. A lamp and millimetre-scale were placed 50 inches from the mirror. As all the observations were expressed in tenths of a millim., to about which extent they can be trusted, it is convenient to employ one scale of numbers of which one tenth millim. is the unit. One complete turn of the mirror is very nearly 160,000 on this scale. If the mirror is moved through 160,000 in either direction and held for one minute, and then allowed to take its new position, the change in the position of rest is as soon as it can be read about 370. This is reduced in about three minutes to 110. If the mirror is

moved through three turns, 480,000 of the scale, and held one minute, the position of rest is at first moved about 1100, which falls in three minutes to about 400. I have given these figures, not because the effect is not perfectly well known, but to serve as comparison figures to those that are to follow. They can only be properly represented on a time-diagram.

A piece of the same fibre that was used in the last experiment was laid in a box of charcoal and heated in a furnace to a dull red heat and allowed to cool slowly. This was examined in the same way as the last. The effect of a movement of 160,000 for one minute was now only about 60, which was reduced to about 45 in three minutes. The change for 480,000 lasting one minute was at first about 250, which fell to about 180 in three minutes.

Annealed spun glass then shows far less of this effect than spun glass not annealed, but it is slower in recovering. It is possible that if time were given, it would show as great an effect as plain glass. The only mineral from which at the present time I have obtained any valuable results in this direction, is quartz. Here the effect of the usual minute at 160,000 was only 7, in the place of 370 for glass, at 320,000 only 17, and 640,000 only 32, which in four minutes fell to 22. This fibre was as usual fastened at each end by sealing-wax. When this experiment was made, the thread had only just been fastened. The same fibre treated previously in the same way, but some days after fastening, did not even show this effect; but as this was before I had completed the proper cell, the observations cannot so well be trusted. After a complete turn, there was not a movement of one tenth of a millim., nor had the position changed this much in 16 hours. It is as yet too soon to be sure, but this seems to point to the possibility of the very slight effect observed being largely due to the sealing-wax. Whether this is so or not does not much matter, the behaviour of the quartz thread approaches sufficiently near to that of an ideal thread, to make it of the utmost value as a torsion-thread. I hope shortly to be able to bring results of carefully conducted experiments on the elastic fatigue of quartz and other fibres before the notice of this Society.

A thread of annealed quartz behaves like a thread of quartz

not annealed. That it was affected by the process of annealing is evident, because in the first place it was very rotten and difficult to handle, and in the second a piece of quartz fibre, which was wound up, retained its form. By this test, quartz can only be partly annealed in a copper box, as any form is not retained perfectly; at a temperature above that of melting copper, quartz seems to perfectly retain any form given to it.

It is probable that a body hung by a fibre of quartz and vibrating in a perfect vacuum would remain twisting backwards and forwards for a far longer time than a similar body hung by a glass thread, also that the most perfect balance-spring for a watch would be one of quartz. I have a piece of quartz drawn out to a narrow neck which just cannot hold up its head; this keeps on nodding in all directions for so long a time, even in the air, as to make it evident that the material has very unusual properties.

3. *Uses.*

As torsion-threads these fibres of quartz would seem to be more perfect in their elasticity than any known; they are as strong as steel, and can be made of any reasonable length perfectly uniform in diameter, and, as already explained, exceedingly fine. The tail-ends of those that become invisible must have a moment of torsion 100 million times less than ordinary spun glass; and though it is impossible to manipulate with those, there is no difficulty with threads less than $\frac{1}{10000}$ inch in diameter.

I have made a spiral spring of glass of about 30 turns which weighs about one milligram; this, examined by a microscope, would show a change in weight of a thing hung by it of one 10 millionth of a gram. Since this has been annealed its elastic fatigue is that of annealed glass, and therefore very small. I have succeeded in doing the same thing with a quartz fibre, but the difficulties of manipulation are very great in consequence of the rottenness of annealed quartz. The glass spring can be pulled out straight, and returns perfectly to its proper form.

Since these fibres can be made finer than any cobweb, it is possible that they may be preferable to spider-lines in eye-

pieces of instruments; they would in any case be permanent, and not droop in certain kinds of weather.

Those who have experienced the trouble which the shifting zero of a thermometer gives, might hope for a thermometer made of quartz. When made, it would probably be more perfect in this respect than a glass thermometer, but the operation of making would be difficult.

These very fine fibres are convenient for supporting small things of which the specific gravity is required, for they weigh nothing, and the line of contact with the surface of the water is so small, that they interfere but little with the proper swing of the balance.

It seemed possible that a diffraction-grating made of fibres side by side in contact with one another would produce spectra which would be brighter than those given by a corresponding grating of ordinary construction, because not only is all the light which falls on the surface brought to a series of linear foci forming the bright lines instead of being half removed, as is usually the case, but the direction of the light on reaching these lines is not normal to the grating as usual, and therefore in the direction of the central image, but spreading, and thus in the direction of all the spectra. I picked out a quantity of glass fibre not varying in diameter more than one per cent., and made a grating in this way covering about one eighth of an inch in breadth. This not only showed three spectra on each side, and a quantity of scattered light, but all the spectra were closely intersected by interference-bands, such as are seen when a Newton's ring of a high order is seen in a spectroscope. This is probably due to a cumulative error in the position of the fibres, for they were spaced by being pushed up to one another with a needle-point, or to light passing between the fibres in a few places where dust particles keep them apart.

A diffraction-grating made of these fibres, spaced with a screw to secure uniformity, and of a thickness equal to the spaces between them (and one of 1000 lines to the inch could be easily made) would be far more perfect for the number of lines than any scratched on a surface; that is, for investigation on the heat of a spectrum, such a grating would be preferable to a scratched one, as there is no uncertainty as to the grating

or to the substance of which it is made *. If the transparency of the fibres interfered they could be rendered opaque by metallic deposit without visibly increasing their diameter.

There is one use to which the fibres of quartz tailing-off to a mere nothing might be applied, namely as a test-object for a microscope. Theory shows that no microscope can truly show any structure much less than $\frac{1}{100000}$ inch, or divide two lines much less than this distance apart. Natural bodies such as Diatoms &c. have this advantage, that they can be obtained in any quantity alike, but no one knows what the real structure of these may be. Nobert's bands are good in that we know the number of lines in any band, but as to the individual appearance of the lines and spaces it is impossible to say anything. These fibres have the advantage that we have a single thing of known form, which tapers down from a definite size to something too small even to be seen. Though it may be possible to calculate the size from the appearance of the fringes, yet whether the size is known or not, at each point we have a definite thing of known form which can be examined by a series of microscopes, and the point up to which it can be clearly seen observed for each.

I have thought it worth while to bring this subject forward in this very incomplete form, because there are already results of interest and there is so much prospect of more, that it is likely that Members may be glad to investigate some of the questions raised.

IV. *Note on Magnetization.—On Sequences of Reversals.* *By* R. H. M. BOSANQUET †.

THE present paper arose out of a recent discussion, at a meeting of the Physical Society, on Prof. Ayrton's paper on the magnetic resistance of a broken ring. I alluded on that occasion to the determinations of the magnetism of rings and bars which I had made. In answer to a question as to the course I had pursued as to ascending or descending values of magnetism, I stated that in all cases I had taken the values in

* See 'Heat,' by Prof. Tait, p. 268.

† Read April 23, 1887.

the ascending order. But I omitted to make it clear that the observations were made by reversal, so that the direct effects of residual magnetism were necessarily eliminated. The arrangement I have always employed may be described as an ascending sequence of reversals.

There can be no doubt, however, that, in sequences of reversals, the history of previous magnetizations produces some effect. I still consider that the best way of attacking the problem is to take the first ascending sequence, in which the history is a minimum, and then as a subsequent study to deal with the effects of the history.

Immediately after the discussion above alluded to I made a few experiments on the first bar I ever completely determined, with the view of ascertaining the general character presented by series of successive reversals. The original determinations will be found at Phil. Mag. (1884) xvii. pp. 531–6, Soft-Iron Bar I.

I shall confine myself at present to the values of magnetic resistance. The reason for preferring this datum is that, when plotted with the induction as abscissa, the form of the curve of magnetic resistance is approximately independent of the arrangement of the metal, the only material differences occurring in the region of saturation. Thus values for bars and rings differ only by a constant, which may be regarded as due to the shape. Bars with pole-pieces have similar curves lying between those for bars and rings. See Phil. Mag. xxii. p. 303, and the paper above referred to.

I shall call the old experiments the first ascending sequence (A). Then I made a series which I shall call the second ascending sequence (B). Then a final descending sequence (C).

The first values of B, which follow the old saturation, have the resistance somewhat higher than that found when the bar was fresh. After a few reversals, however, the effect of the history seems to disappear as the values ascend, and the value for mean inductions (minimum value of resistance) is almost exactly the same as before. The saturation-value corresponds to a higher induction than would be given by the old curve.

The succeeding curve of descending values (C) shows increased resistance, or less magnetism, in the mean inductions.

As the small inductions are approached the curve of C crosses the old curve, and ends with a lower resistance, or greater magnetism than the original initial value.

The effects of ascending and descending sequences of reversals on the initial values would therefore appear to be opposite in direction. At the same time the recent experiments are few in number, and a much more extensive course of work will have to be done before conclusions on these points can be drawn with generality.

I should like to say a few words on the relation of results of this description to the molecular hypothesis, by means of which I have represented a large number of experiments. First, as to the position in which the hypothesis stands.

I am quite unable to understand how Weber's hypothesis can be applied to account for such laws as we are dealing with, *i. e.* where the initial magnetic resistance is greater than that for mean inductions. The hypothesis, which I have worked out and applied in detail to a large number of experimental cases of all sorts*, depends on two chief processes; the one of which accounts chiefly for the larger initial values of the magnetic resistance, the other for the larger saturation-values.

First, as to the saturation-values. Each magnetic particle is supposed to transmit the magnetism through a certain axis and not otherwise. Using the analogy of a hole in a bead, packed with wires, the permeability so far is measured by the portion of the hole left unoccupied, or by what I call the defect of saturation. This alone would lead to a law of permeability similar to Frölich's supposed law of the conductivity of magnets with ends.

Next, to account for the initial values. The axes of the particles being distributed uniformly in all directions, there will be an action arising from the tension of the lines of force tending to draw all the axes towards the mean direction of the magnetism; and thus the length of the path, and consequently the resistance, will be diminished as the magnetism increases. This has all been worked out in detail and shown to be capable

* Phil. Mag. (1885) xix. pp. 73 & 333; xx. p. 318; xxii. p. 298; xxiii. p. 350.

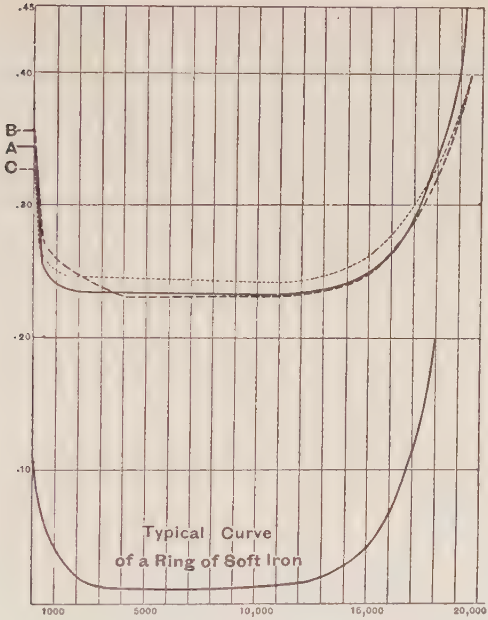
of representing the facts minutely, in the papers referred to. All I want to show here is that we can refer our results as to increase or diminution of resistance by sequences of reversals to this theory. For the permeability would be diminished by partially filling up the saturation-capacity, and increased by partially fixing the molecules in positions of diminished resistance. All magnetization tends to retain the latter change in the same direction ; *i. e.* to diminish the mean inclination of the axes, and consequently the resistance. But successive reversals of diminishing currents are capable of removing the greater part of the magnetism that sticks in the particles (subpermanent magnetism). This accounts generally for the observation that immediately after saturation the resistance is increased, but when a descending sequence of reversals is interposed the resistance is diminished. For the large residual charge after saturation diminishes the permeability to an extent exceeding the effect of inclination of axes. But when the residual charge is dispersed by the reversals of a descending sequence, the effect of the diminished inclination of the axes remains, and increases the initial permeability.

The figure and Tables which follow exhibit the results of the experiments in question.

Soft-Iron Bar : ·938 centim. radius,
28·55 centim. length.

A. Original ascending sequence.		B. Second ascending sequence.		C. Descending sequence.	
𝔅.	ρ.	𝔅.	ρ.	𝔅.	ρ.
	centim.		centim.		centim.
25	·343	14	·356	15	·328
168	·315	106	·342	237	·289
414	·272	258	·314	978	·254
1,423	·237	276	·305	2,529	·250
2,467	·238	384	·286	5,577	·246
10,210	·233	1,244	·257		
17,214	·299	4,384	·231		
17,858	·331	19,396	·385		
19,289	·445				
19,650	·552				

Sequences of Reversals of Magnetism of Soft-iron Bar.
ρ = magnetic resistance, in centimetres.



Bar { A ——— original ascending sequence.
B - - - - - second ascending sequence.
C . . . . . . descending sequence.

The length of the ring is supposed to be the same as that of the bar; *i. e.* 28·55 centim.

V. *On a Magnetic Potentiometer.* *By* A. P. Chattock *.

In arranging some experiments on the magnetic resistances (so-called) of certain air and iron fields with a view to the more satisfactory designing of dynamos, I have been led, by the familiar analogy between magnetic and electrical circuits, to adopt the following method of measurement; which I venture to describe, partly on account of its convenience, partly because the measurement of magnetic resistances seems likely to play an important part in the practical application of electromagnetism.

* Read May 14, 1887.

The resistance between two points on a magnetic circuit may be expressed as the ratio of their potential difference to the total induction passing from one to the other (provided there is no reverse magnetomotive force between them).

The measurement of the total induction is of course a simple matter; but, so far as I am aware, no method of directly measuring differences of potential has yet been suggested.

Let A and B be two points in a magnetic field connected by any line of length l; and let H represent magnetic force resolved along l.

Then, if V be the difference of potential between A and B,

$$V=\int H\,.\,dl.$$

If, instead of points, A and B represent two equal plane surfaces of area a, and $\overline{V}$ be their average difference of potential,

$$a\overline{V}=\int V\,.\,da=\int H.\,dv,$$

v being the volume of a tube of constant cross section, a, connecting A and B by *any* path.

Now let a wire helix be wound uniformly upon such a tube, with n turns per unit length, and allow H to vary with time, t. Provided there be no magnetic substance inside the helix, an electromotive force, E, will be set up in the latter, such that

$$E=\frac{d}{dt}\iint H\,.\,da\,.\,dn=\frac{d}{dt}\,n\int H\,.\,dv=na\frac{d\overline{V}}{dt}.$$

The value of E is thus proportional to the rate of change of $\overline{V}$; and to this *alone*, if external inductive effects are guarded against by winding the wire in an even number of layers (n and a being constant). Hence, if the wire be connected with a ballistic galvanometer, and $\overline{V}$ be altered suddenly from V_1 to V_2, the needle of the galvanometer will be thrown through the angle θ such that

$$V_1-V_2=K\sin\frac{\theta}{2},$$

and the combination forms what may be called a magnetic potentiometer.

In exploring a permanent field with such an apparatus, the best way is, perhaps, to fix one end of the helix in a clip, thereby keeping its potential constant, and to move the other

end from point to point in the field. For this purpose the wire should be wound upon a flexible core, the average length of which, whether bent or straight, must be constant (otherwise $da \,.\, dn$ will not be equal to $n \,.\, dv$ in the last equation).

I have therefore constructed a helix by winding wire uniformly on to a piece of solid indiarubber, of constant cross section (in my case 37 centim. long and about 1 centim. diameter), leaving a small space between one turn and the next to allow the indiarubber to bend without elongating.

With this apparatus I made the following measurements of the potential difference between the ends of a permanent bar-magnet, in order to test the accuracy of the method. In the first set of readings the free end of the helix was moved at one leap from end to end of the magnet, giving a mean reading of 6·017; in the two other sets it was moved between the same two points in two and four leaps respectively, the resulting readings being added together in each case. The means of these were 6·047 and 6·048. The final mean was thus 6·037; and from this the most discordant reading differed by about 1 per cent. The close agreement between the second and third values was, no doubt, accidental; but the difference between them and the first may have been due to the difficulty of moving quickly from end to end of the magnet, a distance of over 40 centim.

In order to reduce these results to absolute measure, the helix (still connected with the galvanometer) was subjected to a known magnetomotive force by passing it through a coil of n turns and bringing its ends close together outside the coil, a current C being then started or stopped in the latter. The magnetomotive force due to this was of course equal to $4\pi n\mathrm{C}$, and this being substituted for $\mathrm{V}_1 - \mathrm{V}_2$ in the last equation determined the value of K. In my case these values were $n = 20$, $\mathrm{C} = 0{\cdot}182$ C.G.S., and galvanometer throw $= 0{\cdot}34$. The difference of potential between the ends of the magnet was thus

$$\frac{4\pi \times 20 \times 0{\cdot}182}{0{\cdot}34} \times 6{\cdot}037 = 810 \text{ C.G.S.}$$

A more suitable core for the helix than indiarubber is the flexible gas-tubing made of plaited and varnished canvas. It is very uniform in cross section; and by withdrawing the

metal spiral upon which it is woven and mounting it on a spindle in a screw-cutting lathe, it is easy to wind the wire uniformly upon it.

The use of the lathe is the more desirable, as measurements of potential by the helix depend very much for their accuracy upon the uniformity with which it is wound ; this being especially the case if its position in the field does not happen to coincide with the direction of the lines of force.

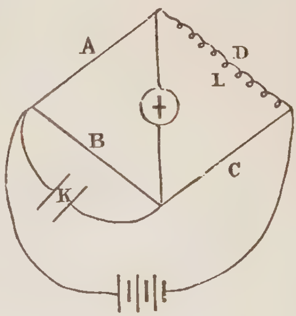
To keep the turns in place the small spaces between them may be filled with soft cotton-thread.

University College, Bristol.

VI. *On a Modification of a Method of* Maxwell's *for Measuring the Coefficient of Self-induction. By* E. C. Rimington *.

In 'Electricity and Magnetism,' § 778, vol. ii., Maxwell gives a method of comparing the coefficient of self-induction of a coil with the capacity of a condenser, the connexions for which are practically as follows :—D is the resistance, possessing self-induction ; the armatures of a condenser, of capacity K, are connected to the ends of the arm B. There are keys in the battery and galvanometer-circuits. An ordinary balance is obtained by depressing the battery-key before the galvanometer-key ; then $AC = BD$. The resistances B and D are next adjusted so that no momentary deflection shall exist on the galvanometer when its circuit is made before that of the battery. Then $L = KBD$.

Fig. 1.



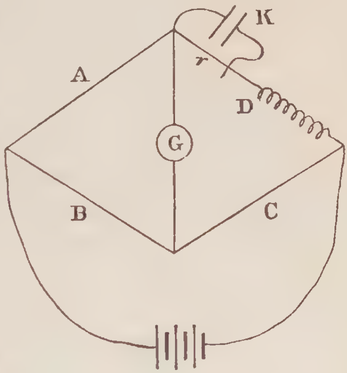
This method is very inconvenient, as it necessitates a double adjustment. One of the resistances, generally C, has to be adjusted so as to give an ordinary balance ; then, in order to obtain no deflection when the galvanometer-circuit is closed,

* Read May 14, 1887.

first the resistance B will have to be altered, as D is generally the resistance of the coil. This necessitates a fresh adjustment of C, and so on.

The first modification of this method I made was by putting a resistance possessing no self-induction in the arm D in series with the coil, as in fig. 2, a slider moving over this resistance and the condenser being connected as shown.

Fig. 2.

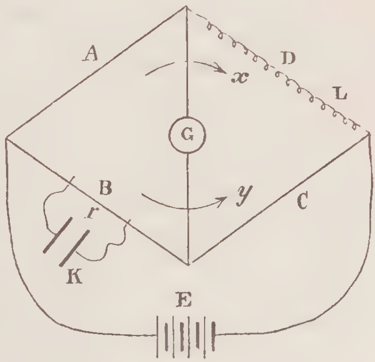


An ordinary bridge-balance is first obtained, and the slider is then adjusted until there is no throw on the galvanometer, when its circuit is closed before that of the battery. Then

$$L = Kr^2,$$

where r is the resistance between the armatures of the condenser. This method was found to be insensitive, as, unless K were large, r had to be made high. It will not do to make K too large with an ordinary mirror-galvanometer, on account of a sort of double throw being obtained, the effect of self-induction being more rapid than that of the condenser, and the galvanometer not ballistic enough. I then adopted the following modification, which answers very well.

Fig. 3.



The arm B is a resistance on which two sliders move, one of them being connected to each armature of the condenser K.

Obtain a permanent balance; then AC=BD.

Now adjust sliders until there is also a balance when galvo.-circuit is closed first. Let x be the current flowing in the arms A and D when it has attained its permanent

value, and let y be that in the arms B and C. Let r be the resistance between the sliders when both balances are obtained. Let the battery and galvanometer circuits be both closed, and let the former be broken. The quantity of electricity which passes through the galvanometer due to the self-induction in D

$$= \frac{\mathrm{L}x}{\mathrm{C}+\mathrm{D}+\dfrac{\mathrm{G}(\mathrm{A}+\mathrm{B})}{\mathrm{G}+\mathrm{A}+\mathrm{B}}} \times \frac{\mathrm{A}+\mathrm{B}}{\mathrm{G}+\mathrm{A}+\mathrm{B}}$$

$$= \frac{\mathrm{L}x\mathrm{B}}{\mathrm{B}(\mathrm{C}+\mathrm{D})+\mathrm{G}(\mathrm{B}+\mathrm{C})}, \text{ since } \mathrm{AC}=\mathrm{BD}.$$

The quantity which passes through the galvanometer, due to the discharge of the condenser,

$$= \mathrm{K}yr\frac{r}{\mathrm{A}+\mathrm{B}+\dfrac{\mathrm{G}(\mathrm{C}+\mathrm{D})}{\mathrm{G}+\mathrm{C}+\mathrm{D}}} \times \frac{\mathrm{C}+\mathrm{D}}{\mathrm{G}+\mathrm{C}+\mathrm{D}}$$

$$= \frac{\mathrm{K}yr^2\mathrm{C}}{\mathrm{B}(\mathrm{C}+\mathrm{D})+\mathrm{G}(\mathrm{B}+\mathrm{C})}, \text{ since } \mathrm{AC}=\mathrm{BD}.$$

Now these quantities obviously pass through the galvanometer in opposite directions; and if there is no throw, they must be equal. Therefore

$$\frac{\mathrm{L}x\mathrm{B}}{\mathrm{B}(\mathrm{C}+\mathrm{D})+\mathrm{G}(\mathrm{B}+\mathrm{C})} = \frac{\mathrm{K}yr^2\mathrm{C}}{\mathrm{B}(\mathrm{C}+\mathrm{D})+\mathrm{G}(\mathrm{B}+\mathrm{C})},$$

or

$$\mathrm{L} = \mathrm{K}r^2\frac{y}{x}\cdot\frac{\mathrm{C}}{\mathrm{B}}.$$

Now $\dfrac{y}{x} = \dfrac{\mathrm{D}}{\mathrm{C}}$; $\therefore$ $\mathrm{L} = \mathrm{K}r^2\dfrac{\mathrm{D}}{\mathrm{B}}$.

If $r=\mathrm{B}$, we have Maxwell's method, and

$$\mathrm{L} = \mathrm{KBD}.$$

Of course it is not necessary to use two sliders; one armature of the condenser can be connected either to the junction of A and B or to the junction of B and C, and the other to the slider; but by having B composed of two slide-resistances, the smaller one being equal to the resistance of

one of the coils of the larger, and employing a slider on each, a much greater range of adjustment can be obtained.

Suppose r to be slightly out of adjustment by an amount δ. Then the quantity which passes through the galvanometer, or

$$q=\frac{\mathrm{K}(r+\delta)^2\mathrm{C}y-\mathrm{BL}x}{\mathrm{B(C+D)+G(B+C)}}.$$

Now $y=x\frac{\mathrm{D}}{\mathrm{C}}$;

$$\therefore q=x\frac{\mathrm{K}(r+\delta)^2\mathrm{D}-\mathrm{BL}}{\mathrm{B(C+D)+G(B+C)}},$$

and since $\mathrm{BL}=\mathrm{K}r^2\mathrm{D}$,

$$q=x\frac{2\mathrm{K}r\delta\mathrm{D}}{\mathrm{B(C+D)+G(B+C)}}.$$

If p be the fractional error in r, $\delta=pr$;

$$\therefore\ q=x\frac{2\mathrm{K}r^2\mathrm{D}p}{\mathrm{B(C+D)+G(B+C)}}.$$

Now

$$x=\frac{\mathrm{E}}{\rho+\frac{\mathrm{(A+D)(B+C)}}{\mathrm{A+B+C+D}}}\times\frac{\mathrm{C}}{\mathrm{C+D}}$$

$$=\mathrm{E}\frac{\mathrm{C}}{\rho(\mathrm{C+D})+\mathrm{D(B+C)}},$$

since $\mathrm{AC}=\mathrm{BD}$; and $\mathrm{K}r^2\mathrm{D}=\mathrm{BL}$;

$$\therefore\ q=\frac{2\mathrm{EL}p\mathrm{BC}}{\{\rho(\mathrm{C+D})+\mathrm{D(B+C)}\}\{\mathrm{B(C+D)+G(B+C)}\}}$$

$$q=\frac{2\mathrm{E}lp}{\left\{\rho\left(1+\frac{\mathrm{D}}{\mathrm{C}}\right)+\mathrm{D}\left(1+\frac{\mathrm{B}}{\mathrm{C}}\right)\right\}\left\{\mathrm{C+D+G}\left(1+\frac{\mathrm{C}}{\mathrm{B}}\right)\right\}}.$$

If we wish to make q as large as possible for a given value of p, we must make C small and consequently B also small. Of course B cannot be less than r, and will generally be a given resistance, depending on the apparatus we are employing. If B is given, differentiating the expression for q with respect to C, and equating to zero, we obtain

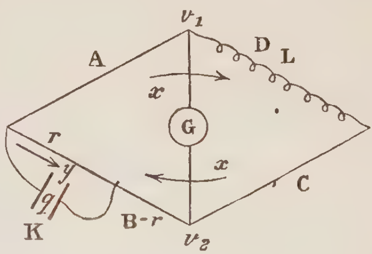
$$\mathrm{C}=\sqrt{\frac{\mathrm{BD(G+D)}(\rho+\mathrm{B})}{\mathrm{(G+B)}(\rho+\mathrm{D})}}.$$

If ρ is small compared to the other resistances, this gives

$$C = B\sqrt{\frac{G+D}{G+B}}.$$

This method can be made much more sensitive by adopting the principle employed by Professors Ayrton and Perry in their admirable instrument the Secohmmeter. A commutator is introduced into the battery and galvanometer-circuits, which by its revolution puts on the battery, the galvanometer-circuit being closed, breaks the latter, or short-circuits the galvanometer and then breaks the battery-circuit, afterwards closing the galvanometer-circuit, repeating this cycle of operations during each revolution. In this method neither the speed of the commutator nor the fraction of the cycle during which the battery and galvanometer are on together need be known, as is the case with the secohmmeter.

To find the conditions under which a telephone may replace the galvanometer.



In order that the galvanometer may be replaced by a telephone it is necessary that the current through it at any moment shall be zero. Consequently v_1 must equal v_2 always. Therefore the resistance of G may be anything. Let it be infinite, and let x be the current in A, D, C, and B$-r$, y the current in r, and q the charge on the condenser at any moment t after the battery-circuit is broken.

Let x_0 be the current in D, y_0 the current in r, and q_0 the charge on the condenser at the moment of breaking the battery-circuit, and suppose the break is not prolonged by a spark at the contact.

Then

$$(C+D)x + L\frac{dx}{dt} = v_1 - v_2 = 0, \quad . \quad . \quad . \quad . \quad (1)$$

$$(A+B-r)x - ry = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$x + y = -\frac{dq}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

From (2),

$$y = x\frac{A+B-r}{r};$$

and therefore from (3),

$$x\frac{A+B}{r} = -\frac{dq}{dt}. \qquad \qquad (4)$$

Now from (1),

$$\frac{dx}{x} = -\frac{C+D}{L}dt,$$

$$\int_{x_0}^{x}\frac{dx}{x} = -\frac{C+D}{L}\int_0^t dt;$$

or

$$\log\frac{x}{x_0} = -\frac{C+D}{L}t,$$

$$x = x_0 e^{-\frac{C+D}{L}t}.$$

Again, since the resistance of G may be anything, let it equal zero; then the condenser is discharging through a resistance equal to

$$\frac{r(A+B-r)}{A+B} = \frac{r\{B(C+D)-Cr\}}{B(C+D)},$$

since

$$AC = BD.$$

Therefore

$$q = q_0 e^{-\frac{t}{K\frac{r\{B(C+D)-Cr\}}{B(C+D)}}} = q_0 e^{-\frac{t}{K}\cdot\frac{B(C+D)}{r\{B(C+D)-Cr\}}},$$

and

$$-\frac{dq}{dt} = q_0\frac{B(C+D)}{Kr\{B(C+D)-Cr\}}e^{-\frac{t}{K}\cdot\frac{B(C+D)}{r\{B(C+D)-Cr\}}}.$$

Also

$$q_0 = Ky_0 r \quad \text{and} \quad \frac{A+B}{r} = \frac{B(C+D)}{Cr}.$$

Substituting these values in (4), we obtain

$$x_0\frac{B(C+D)}{Cr}e^{-\frac{C+D}{L}t} = y_0\cdot\frac{B(C+D)}{B(C+D)-Cr}\cdot e^{-\frac{B(C+D)}{Kr\{B(C+D)-Cr\}}t}.$$

Also

$$\frac{x_0}{y_0}=\frac{C}{D};$$

therefore

$$\frac{1}{r}e^{-\frac{C+D}{L}t}=\frac{D}{B(C+D)-Cr}e^{-\frac{B(C+D)}{Kr\{B(C+D)-Cr\}}t}.$$

This equation must hold for every value of t; hence

$$\frac{1}{r}=\frac{D}{B(C+D)-Cr}, \quad \ldots \quad (A)$$

and

$$\frac{C+D}{L}=\frac{B(C+D)}{Kr\{B(C+D)-Cr\}}. \quad \ldots \quad (B)$$

From (A),

$$rD=B(C+D)-Cr,$$

$$r(C+D)=B(C+D);$$

$$\therefore\ r=B \quad \text{or} \quad C=-D.$$

From (B),

$$L=\frac{Kr\{B(C+D)-Cr\}}{B}$$

$$=Kr\left(C+D-r\frac{C}{B}\right).$$

If $r=B$, $\quad L=KBD.$

If $C=-D$, $\quad L=Kr^2\frac{D}{B}.$

As $r=B$ is the only solution possible, the method can only be used with a telephone under the conditions given by Maxwell, and is impracticable on account of the trouble met with in obtaining the double adjustment.

VII. *On Evaporation and Dissociation.*—Part VI. (*continued*). *On the Continuous Change from the Gaseous to the Liquid State at all Temperatures.* *By* WILLIAM RAMSAY, *Ph.D.*, *and* SYDNEY YOUNG, *D.Sc.**

[Plates I.–III.]

THE following pages give a further proof of the correctness of the relation $p = bt - a$, where v = constant, applicable both to gases and liquids. The data for methyl alcohol apply solely to the gaseous state, for the very high pressures which its vapour exerts precluded measurements at temperatures above its critical point. With ethyl alcohol the determinations of the compressibility of the liquid are more complete than with ether; the experimental observations in the neighbourhood of the critical volume are, however, not very numerous, for the highest temperature for which an isothermal was constructed is 246°, the critical temperature being 243°·1. The values of a and b at volumes near the critical are consequently somewhat uncertain. The data for the gaseous condition are, however, pretty full. We have also a considerable number of data for acetic acid (Trans. Chem. Soc. 1886, p. 790). Here the temperature at which the highest isothermal was measured was the highest conveniently attainable by our method, viz. 280°. But as the critical temperature lies much higher, the pressures were in no case very great. The behaviour of acetic acid, however, contrasted with that of the alcohols, ether, and carbon dioxide is very striking. The equation $p = bt - a$ does not apply; in other words lines of equal volume are not straight, but are curves of double flexure. We shall consider the meaning of this peculiarity after adducing data.

1. *Methyl Alcohol.*—The data are at present in the hands of the Royal Society. The values of b were, as before, determined by reading points on the isothermal curves at equal volumes; constructing isochors graphically, and having thus obtained approximate values of b, these were smoothed by

* Read April 23, 1887.

plotting them as abscissæ, the ordinates being the reciprocals of the volumes. The values of b given in the Table which follows were those read from this curve. The values of a were calculated from the equation $a=bt-p$, the mean value obtained from all the readings at each volume being taken as correct.

TABLE I.

Vol.	b.	log b.	a.
c. c. per gram.			
7	626·5	2·79692	263430
8	569·0	2·75511	235370
9	509·0	2·70672	206290
10	452·5	2·65562	179090
11	405·3	2·60778	156660
12	365·0	2·56229	137810
14	299·0	2·47567	107460
16	248·5	2·39533	84730
18	211·0	2·32428	68364
20	184·8	2·26670	57474
25	139·8	2·14551	39724
30	112·5	2·05115	29834
40	80·0	1·90309	18913
50	62·0	1·79239	13401
70	43·5	1·63849	8631
100	26·95	1·43056	4092
135	18·95	1·27761	2398
170	14·50	1·06137	1624
200	12·00	1·07918	1187
240	9·65	0·98453	794
280	8·00	0·90309	455
340	6·30	0·79934	307
400	5·15	0·71181	159
450	4·57	0·65992	130

The following Table gives the pressures read from the isothermals from which these values of a and b were obtained; and we have added, for the sake of comparison, the pressures recalculated by help of these values.

Table II.

Vol. c. c. per gram	Temperatures. 240°. Read.	240°. Calc.	239°·5. Read.	239°·5. Calc.	239°. Read.	239°. Calc.	238°. Read.	238°. Calc.	236°. Read.	236°. Calc.	234°. Read.	234°. Calc.	232°. Read.	232°. Calc.	230°. Read.	230°. Calc.
	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
7	58220	57970	57700	57650	57280	57340	56660	56710	55250	55460	54200	54210				
8	56660	56530	56260	56240	55930	55960	55330	55390	54050	54250	53190	53110	52060	51570		
9	54990	54830	54580	54570	54240	54320	53790	53810	52640	52790	51910	51780	50800	50760	49630	49740
10	53200	53040	52860	52810	52600	52590	52040	52140	51180	51240	50390	50330	49430	49420	48370	48520
11	51390	51260	51160	51060	50870	50860	50320	50450	49640	49640	48780	48830	48000	48020	47040	47210
12	49450	49430	49380	49250	49150	49070	48610	48700	48070	47980	47190	47250	46530	46510	45710	45790
14	45980	45930			45670	45630	45260	45330	44800	44730	44150	44130	43620	43530	42930	42940
16	42800	42750			42570	42500	42190	42260	41780	41760	41250	41260	40840	40760	40200	40270
18	39920	39876			39660	39666	39400	39456	39000	39036	38600	38616	38250	38186	37740	37766
20	37360	37328			37110	37143	36900	36958	36600	36589	36205	36220	35940	35849	35400	35481
25	32000	31994			31810	31854	31670	31714	31440	31435	31120	31156	30900	30876	30600	30596
30	27845	27878			27805	27766	27630	27653	27420	27429	27200	27204	27000	26978	26795	26753
40	22065	22127			22000	22047	21930	21967	21820	21807	21650	21647	21510	21487	21375	21327
50	18380	18405													17820	17785
70	13590	13685													13210	13250
100	9690	9733													9460	9464
135	7300	7323													7180	7134
170	5800	5815													5679	5670
200	4960	4969													4856	4849
240	4153	4156													4064	4060
280	3558	3559													3494	3478
340	2921	2925													2873	2862
400	2491	2483													2442	2431
450	2211	2214													2178	2169

Table II. (*continued*).

Vol. c. c. per gram.	Temperatures.													
	225°.		220°.		200°.		180°.		160°.		120°.		100°.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
10	46260	46260												
11	45280	45180												
12	43980	43960	42000	42140										
14	41400	41440	39810	39950										
16	38990	39020	37740	37780										
18	36740	36716	35660	35656										
20	34600	34556	33660	33633										
25	29970	29897	29210	29198										
30	26200	26191	25700	25629	23790	23379	20600	21129						
40	20960	20927	20620	20527	19060	18927	17140	17327						
50					15980	15925	14600	14685						
70					11980	11945	11140	11075	10240	10205				
100					8640	8655	8200	8117	7605	7577				
135					6520	6565			5830	5808				
170					5221	5235			4696	4655	4056	4075		
200					4467	4489			4040	4009	3522	3529		
240					3748	3770			3404	3375	2999	2999		
280					3229	3239			2939	2919	2605	2599	2405	2439
340					2670	2673			2435	2421	2172	2169	2022	2043
400					2274	2277			2079	2071	1863	1865	1740	1762
450					2024	2030			1860	1849	1665	1666	1565	1575

It is evident that the agreement here is very satisfactory, and that any differences are well within the limits of experimental error, and of error in reading from curves.

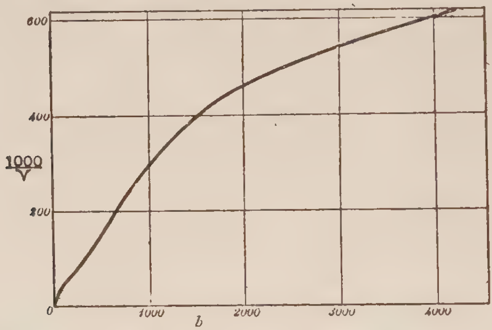
2. *Ethyl Alcohol.*—The data for ethyl alcohol are published in the 'Philosophical Transactions,' 1886, part i. p. 123. In constructing isothermals for the liquid, which are published in plate iii. of that memoir, isobars were constructed from the isothermal curves drawn through the experimental points; these isobars were made use of in finding isochoric lines at which the relations of temperature and pressure could be compared. This method applies to volumes not greater than 3 cub. centim. per gram. Between volumes 3 and 12 cub. centim. per gram the isochoric lines were constructed from the diagram from which plate vi. was copied (the smaller divisions are omitted on the printed plate). For volumes above 12 cub. centim. per gram the actual observations were made use of which are detailed on pp. 144–151; the slight alteration in volume caused by the expansion of the measuring-tube was here neglected; at the utmost the error introduced is 0·15 per cent. of the total volume.

The only satisfactory manner of comparing the found and calculated pressures for liquid alcohol is to reproduce the diagram showing the compressibility of the liquid, denoting the calculated pressures by crosses, the actual experimental observations being represented by circles. Although there is apparently only one volume at which a comparison is possible on each of the lower isotherms, yet it must be remembered that the values of a and b are deduced from the isobaric curves, to construct which numerous points on each isotherm were made use of.

Table III. on page 39 gives the values of a and b for definite volumes. These were read from three overlapping curves, on which values of b were abscissæ, the reciprocals of the volumes being ordinates; the actual values of b in the region of the critical volume were, for reasons previously given, interpolated. The form of this curve is remarkable. It is equivalent to the first term in Clausius's and Van der Waals's formulæ, $\frac{RT}{(v-\beta)}$; but it is evident that its form cannot be represented so simply. Its general form is represented in the accompanying woodcut.

The same general form is to be noticed in the case of ether. The curve obtained in plotting a against volume is of similar form.

The comparison of actual and calculated results at volumes below 3 cub. centim. per gram is difficult. As it is most



important to show the truth of the equation $p=bt-a$ for liquids, we have decided, after due consideration, to reproduce the isothermals exhibiting compressibility, inserting crosses (×) to denote the pressures calculated by means of the values of b and a given in the preceding Table. It will be noticed that these crosses are often more in concordance with the actual observations than are the lines drawn before we had discovered this relation. These are shown on Plate I.

Between volumes 3 and 12 cub. centim. per gram we give a table of comparison (Table IV.).

For higher volumes a comparison is made with the results of actual experiment. The volume at the mean temperature is given in each case, although at the extreme temperatures the divergence in some cases amounts to 0·15 per cent. (Table V.).

The values of a and b given are the best available from the determinations; but as these are few at volumes near the critical volume, the range of temperature being too small to permit of ascertaining the values with accuracy, no great dependence can be placed on them. This is proved by the fact that at 220°, if the areas included between the vapour-pressure line and the serpentine curve are made equal, the vapour-pressure shows an error of about 3 per cent. We do not regard this as in any way opposed to our conclusions; for while with ether about the critical volume, the range of temperature from which the values of b and a were calculated

amounted to 55 degrees, and while at volumes slightly greater measurements up to 85 degrees were made use of, with alcohol the total range of temperature was only 3 degrees at corresponding volumes. They cannot, therefore, have at all the same claim to accuracy.

Acetic Acid.—The data are given in the Trans. Chem. Soc. 1886, p. 790. As in the original paper we have omitted to state the volumes of 1 gram, we give a Table (VI.) containing these values.

TABLE III.

Volume.	*b*.	log *b*.	*a*.
c. c. per gram.			
1·4	6295	3·79900	2376800
1·45	5725	3·75778	2270060
1·5	5250	3·72016	2164680
1·55	4800	3·68124	2041900
1·6	4410	3·64444	1925370
1·65	4075	3·61013	1817760
1·7	3750	3·57403	1702370
1·75	3490	3·54283	1608180
1·8	3240	3·51054	1511410
1·85	3000	3·47712	1413870
1·9	2795	3·44638	1327920
1·95	2600	3·41497	1242030
2·0	2435	3·38650	1168030
2·1	2150	3·33244	1037000
2·2	1940	3 28780	938360
2·3	1765	3·24674	854080
2·4	1640	3·21484	793230
2·6	1435	3·15685	691280
2·74	1330	3·12385	638080
3·0	1170	3·06819	556040
3·5	977	2·98989	456490
4·0	842	2·92531	386810
4·5	741	2·86982	334710
5	662	2·82086	294080
6	556	2·74507	240150
7	484	2·68484	204010
8	428·5	2·63195	176650
9	381	2·58092	153610
10	341·2	2·53301	134650
12	276·5	2·44169	104590
14·28	221	2·34439	79460
18·25	155	2·19033	50530
22·13	113·7	2·05576	33080
26·08	88·4	1·94645	23120
30·0	72·1	1·85793	17030
37·8	52·4	1·71933	10470
53·45	34·2	1·53403	5400
76·6	22·6	1·35411	2902
108·2	15·3	1·18469	1565
131·7	12·7	1·10382	1282
170·5	9·6	0·98227	886
484	3·3	0·51851	204

TABLE IV.

Vol.	Temperatures.																			
	246°.		244°·35.		243°·55.		243·1°.		242°.		238°.		236°.		234°.		232°.		230°.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
c. c. p. gr.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
3·5	50180	50580	49100	48950	48400	48060	47640	47730												
4·0	50000	50190	48960	48790	48270	48120	47610	47740												
4·5	49840	49870	48730	48640	48180	48050	47520	47720												
5·0	49480	49500	48350	48400	48110	47880	47300	47580	46970	46850										
6·0	48420	48410	47460	47490	46670	46800	46210	46190	45210	45080										
7·0	47060	47180	46320	46380	45760	45780	45270	45250	44350	44280	43400	43310								
8·0	45500	45740	44920	45030	44440	44500	43030	44030	43220	43170	42450	42310	41600	41460	40640	40600				
9·0	43860	44130	43350	43500	42930	43020	42550	42600	41880	41840	41210	41080	40470	40320	39690	39560	38860	38790	38050	38030
10·0	42130	42430	41680	41870	41350	41440	40990	41070	40420	40390	39800	39900	39150	39020	38480	38340	37810	37660	37110	36980
12·0	38700	38910	38320	38460	38050	38110	37780	37810	37270	37250	36780	36700	36250	36150	35710	35600	35130	35040	34550	34490

TABLE V.

Temperature.	Volumes.											
	14·28 c. c. per gram.		18·25 c. c. per gram.		22·13 c. c. per gram.		26·08 c. c. per gram.		30·0 c. c. per gram.		37·8 c. c. per gram.	
	Pressures.		Pressures.		Pressures.		Pressures.		Pressures.		Pressures.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
°	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
246	34928	35240	29929	29915	25936	25931	......	......	20430	20390	16817	16726
244·35	34657	34870	29891	29658	25664	25651	......	......	20269	20270	16704	16639
243·1	34546	34600	29438	29465	25570	25600	......	......	20158	20180	16611	16573
242·0	34093	34360	29372	29295	25554	25476	......	......	20216	20101	16566	16516
241·0	34150	34130	29277	29139	25482	25361	......	......	20013	20029	16535	16463
240·0	33900	33910	29037	28985	25303	25248	......	......	19959	19957	16500	16410
239·0	33890	33690	28785	28830	25262	25134	......	......	20000	19885	16408	16359
238	33767	33470	28746	28675	24988	25021	......	......	19910	19813	16322	16306
237	33338	33250	28612	28520	24980	24907	......	......	19802	19748	16330	16254
236	33106	33030	28371	28365	24872	24794	......	......	19694	19669	16249	16202
234	32721	32590	27740	28055	24504	24566	......	......	19458	19524	16135	16097
232	32091	32140	27448	27745	24100	24338	......	......	19108	19380	15759	15992
230	31636	31700	27144	27435	23827	24111	21190	21345	19082	18975	15733	15887
225	30601	30600	26546	26660	23365	23543	20809	20903	18729	18875	15436	15625
220	29632	29490	......	......	22964	22975	......	......	18355	18515	15214	15363
210	......	......	24723	24335	21992	21837	19769	19577	17899	17794	14912	14839
200	......	......	......	......	20814	20700	......	......	17140	17073	14444	14315
190	......	......	......	......	......	......	17846	17809	16364	16352	13805	13791
180	......	......	......	......	......	......	......	......	......	......	13183	13267

Table V. (*continued*).

Temperature.	Volumes.											
	53·45 c. c. per gram.		76·6 c. c. per gram.		108·2 c. c. per gram.		131·7 c. c. per gram.		170·5 c. c. per gram.		484 c. c. per gram.	
	Pressures.		Pressures.		Pressures.		Pressures.		Pressures.		Pressures.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
°	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
246	12381	12350	......	......	......	......	5276	5309	4091	4096	......	......
244·35	12313	12293	......	......	......	......	5259	5288	4090	4080	......	......
243·1	12260	12251	8793	8762	6332	6331	......	......	4053	4069	......	......
242·0	12238	12213	......	......	......	......	5216	5258	4062	4058	......	......
241·0	12207	12179	......	......	......	......	5212	5246	4055	4048	......	......
240·0	12188	12145	......	......	......	......	5198	5233	4046	4039	......	......
239·0	12161	12111	......	......	......	......	5185	5220	4029	4029	......	......
238	12087	12076	8673	8647	6274	6253	......	......	4030	4020	......	......
237	12094	12042	8676	8624	......	......	......	......	4022	4010	......	......
236	12073	12008	8669	8602	6249	6223	......	......	4029	4000	......	......
234	11969	11940	8628	8556	6210	6192	......	......	3999	3981	......	......
232	......	......	......	......	6089	6161	......	......	3935	3962	......	......
230	11635	11803	......	......	......	......	......	......	3898	3943	......	......
225	11488	11632	8251	8353	5968	6055	......	......	3863	3895	......	......
220	11366	11461	8168	8240	5916	5978	......	......	3823	3847	......	......
210	11193	11119	......	......	......	......	4873	4852	......	......	......	......
200	10863	10777	......	......	......	......	4758	4725	......	......	......	......
190	10553	10435	......	......	......	......	4647	4598	......	......	......	......
180	......	......	......	......	5400	5366	......	......	......	......	......	......
170	9704	9751	7131	7110	5261	5213	......	......	......	......	......	......
160	9254	9409	6915	6884	5105	5060	4272	4217	......	......	......	......
150	......	......	6632	6658	4933	4907	4128	4090	......	......	......	......
140	......	......	......	......	4749	4754	3978	3963	......	......	......	......
130	......	......	......	......	......	......	3838	3836	......	......	......	......
120	......	......	......	......	......	......	......	......	2942	2887	......	......
110	......	......	......	......	......	......	......	......	......	......	......	......

Table VI.

Temp. 50°.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
13·4	15100
15·4	13300
16·95	11979
18·2	10640
21·0	9325
24·15	8000
26·5	7101
30·0	6214
34·9	5329
51·35	3567

Temp. 78°·4.	
18·8	14550
19·3	14760
20·95	13320
21·1	13250
21·85	12430
22·85	11980
23·35	12000
23·45	11550
24·9	10660
27·0	9774
27·15	9760
27·3	9858
29·3	8868
31·8	7962
32·7	7833
35·85	7092
40·15	6217
42·05	6075
46·4	5326
52·9	4631
54·35	4441
63·2	3865
66·75	3553
71·7	3281
87·2	2670
91·2	2496
101·25	2220
106·6	2132
121·5	1826
124·65	1782
125·3	1767
141·3	1519
151·7	1421
157·75	1375
164·2	1306
172·25	1243
180·8	1175
191·75	1066

Temp. 92°.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
20·45	14650
24·55	11990
30·58	9325
39·35	7105
51·1	5329
66·45	4000
75·65	3515
96·8	2649
104·2	2440
135·8	1825
138·6	1792
170·5	1403
181·05	1327
234·9	982
268·4	843

Temp. 105°·1.	
21·85	15150
31·9	10210
53·6	5782
74·65	4005
83·45	3590
91·4	3110
106·3	2721
126·3	2220
150·4	1823
155·4	1778
186·2	1431
199·75	1334
232·6	1123
286·5	884·6
298·6	841·5
337·4	729·3
422·4	561·0
460·35	504·9

Temp. 117°·95.	
75·75	4488
85·35	3871
163·5	1868
210·45	1403
256·4	1121
328·6	841·5
386·1	701·3
467·1	561·0
601·8	420·8
632·5	392·7
676·8	364·7

Temp. 118°·2.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
24·1	15100
29·1	12430
36·65	9769
44·0	7984
55·3	6217
69·25	4885
84·05	4010
88·6	3782
121·5	2664
172·6	1781
224·2	1335
319·0	876·6
391·9	689·2
433·6	611·0

Temp. 132°·9.	
101·7	3591
132·7	2665
174·5	1969
214·9	1546
285·2	1122
360·4	851·3
470·7	617·1
700·2	391·3
747·6	364·7

Temp. 147°·6.	
88·5	4516
133·9	2805
181·6	2045
225·1	1627
263·5	1352
341·8	1010
427·9	785·4
507·0	645·2
602·5	533·0
729·7	420·8

Temp. 162°·5.	
95·9	4460
134·3	3142
183·8	2244
274·0	1459
373·1	1038
450·5	841·5

Table VI. (*continued*).

Temp. 162°·5 (*cont.*).	
Pressure.	Vol. of 1 gram.
millim.	c. c.
526·7	701·3
640·3	561·0
729·5	476·9

Temp. 184°·1.	
98·05	4654
140·7	3223
188·2	2384
242·9	1823
336·8	1290
425·7	1010
520·4	813·5
666·4	617·2
791·6	504·9

Temp. 162°·5.	
1413	217·1
1523	197·3
1616	184·1
1777	164·0
1907	150·5
2049	137·3
2128	130·6
2212	124·0
2291	117·4
2390	110·7
2456	104·1

Temp. 183°·7.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
1630	216·8
1764	197·0
1876	183·8
2066	163·8
2299	143·7
2599	123·8
2993	103·9
3511	84·17
3733	77·64

Temp. 200°.	
1780	216·8
1984	190·5
2341	157·1
2852	123·9
3679	90·75
4452	71·14
5178	58·15

Temp. 240°.	
2139	217·5
2389	191·1
2851	157·6
3512	124·2
4578	91·02
5612	71·36
6604	58·33
8043	45·27
9006	38·72
10295	32·16
11930	25·60

Temp. 279°·85.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
20669	15·82
21043	15·34
21444	14·86
21762	14·38
22147	13·90
22565	13·42
22993	12·93
23383	12·44
23907	11·95
24075	11·70
24260	11·46

Temp. 280°·4.	
2400	217·5
2475	211·1
2556	204·5
2639	197·9
2731	191·3
2824	184·6
3284	157·7
4085	124·4
5383	91·12
6661	71·44
8742	51·87
11048	38·77
12742	32·20
15105	25·63
18496	19·04
20693	15·74
21848	14·42
23433	12·44

These numbers were mapped on curve-paper, and pressures were read from the curves, corresponding to even volumes.

TABLE VII.

Vol.	50°.	78°·4.	92°.	105°·1.	118°·1.	132°·9	147°·6	162°·5.	184°·1.
c. c.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	m m .	mm.
15000	13·85	18·7	20·0	22·0	24·3				
8000	24·00	32·25	35·15	39·65	43·9				
4000	45·45	60·4	66·55	75·1	83 65	93·0	97·7	106·0	114·7
2000	......	111·0	124·9	140·3	155·0	171·5	185·5	204·5	221·5
1000	......	205·0	231·2	257·5	284·5	315·2	345·0	385·0	430·0
500	......	......	......	469·7	517·5	563·0	635·0	700·5	802·5

Table VII. (*continued*).

Vol.	162°·5.	183°·7.	200°.	240°.	280°.
c. c.	mm.	mm.	mm.	mm.	mm.
220 ...	1405	1610	1755	2115	2380
150 ...	1910	2225	2435	2985	3440
100 ...	2545	3080	3400	4235	4962
80 ...	......	3670	4055	5105	6030
60 ...	.....	......	5057	6465	7750

Before discussing these results we give similar tables calculated and read from numbers given in the Messrs. Natansons's memoir on nitric peroxide (Wiedemann's *Annalen*, 1886, p. 606). As their results are stated as vapour-densities, we have converted their numbers into volumes of 1 gram.

Table VIII.

Temp. −12°·6.

Pressure.	Vol. of 1 gram.
millim.	c. c.
115·4	1648

Temp. 0°.

Pressure.	Vol. of 1 gram.
37·96	6235
86·57	2539
172·48	1208
250·66	808·1

Temp. 21°.

Pressure.	Vol. of 1 gram.
491·60	479·8
516·96	453·0
556·50	419·4
619·17	360·6

Temp. 49°·7.

Pressure.	Vol. of 1 gram.
26·8	15585
93·75	4144
182·7	2008
261·4	1354
497·75	651

Temp. 73°·7.

Pressure.	Vol. of 1 gram.
millim.	c. c.
49·65	9214
64·75	7017
67·72	6754
107·47	4184
164·59	2701
302·04	1420
504·14	816·8
633·27	635·9

Temp. 99°·8.

Pressure.	Vol. of 1 gram.
11·73	41542
23·22	20993
34·80	13929
57·35	8468
79·57	6067
89·67	5408
108·65	4454
116·58	4156
142·29	3402
202·24	2375
371·27	1277
520·98	900

Temp. 99°·8 (*cont.*).

Pressure.	Vol. of 1 gram.
658·31	705·5
675·38	687·7
732·51	629·7

Temp. 117°·91.

Pressure.	Vol. of 1 gram.
58·24	8972·3

Temp. 129°·9.

Pressure.	Vol. of 1 gram.
35·99	15076
66·94	8100
78·73	6892
104·77	5182
152·46	3568
169·71	3194
247·86	2186
297·95	1823
550·29	980·1

Temp. 151°·4.

Pressure.	Vol. of 1 gram.
117·98	4869
475·41	1210
666·22	861·1

These numbers, after mapping, gave the following pressures for even volumes :—

TABLE IX.

Volumes.	−12°·6.	0°.	49°·7.	73°·7.	99°·8.	129°·9.	151°·4.
c. c.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
16000	......	......	26·3	......	30·0	34·5	
8000	......	30·7	50·8	56·5	61·2	68·8	
4000	......	58·5	96·2	112·5	121·2	136·0	146·3
2000	97·0	110·0	183·7	220·0	241·0	271·3	294·0
1000	......	204·2	353·0	422·5	473·5	540·0	573·5

On comparing the results for acetic acid and for nitric peroxide with those for methyl and ethyl alcohols, ether, and carbonic anhydride, the most noticeable feature is that the isochoric lines are not straight. If it is true that the limiting formulæ for acetic acid and for nitric peroxide are, respectively, $C_2H_4O_2$ and $C_4H_8O_4$, and NO_2 and N_2O_4*, then, on the supposition that the lower formulæ remain correct, and that the density remains constant, it is possible to calculate pressures and temperatures corresponding to given isochoric lines; these lines would be those of a perfect gas, straight, and radiating from absolute zero of temperature and pressure; and the isochoric line corresponding to a volume v of the simpler molecule $C_2H_4O_2$, or NO_2, will be the same as the isochoric line for the formula $C_4H_8O_4$ or N_2O_4, and the volume $2v$. Now as the density of the more complex molecule diminishes with rise of temperature and pressure, it is to be expected that the isochoric line should lie between these lines, and that it should coincide at high temperatures with the isochoric line v, and at low temperatures with the line $2v$.

This will be readily understood on reference to the accompanying plates. Plate I. represents isochoric lines for acetic acid, and Plate II. for nitric peroxide. Take the volume 2000 cub. centims. per gram. The line AB represents the relations of temperature to pressure on the assumption that the formula of acetic acid is $C_2H_4O_2$, and its corresponding density 30. The line EF represents similar relations on the assumption that the formula is $C_4H_8O_4$, and the density 60.

* We must here direct attention to an arithmetical mistake in our former paper, "On the Nature of Liquids" (Proc. Phys. Soc. vol. viii. p. 136). The density of nitric peroxide at −12°·6 is stated to be 52·54, it should be 42·54; and the deductions from that erroneous statement must therefore be withdrawn.

This line, it will be seen, also corresponds to a volume of 4000 cub. centims. of gas of density 30. The line CD represents the observed relations of temperature to pressure for acetic acid when the volume is 2000 cub. centims. per gram. It is a curve of double flexure, which, if produced towards B, apparently might touch and become continuous with the line AB ; and if produced towards E might coincide with the line EF. For these large volumes of gas, it is almost certain that the density would become normal at a sufficiently high temperature ; but at low temperatures and pressures it cannot be proved, owing to condensation, whether the molecular formula would be $C_4H_8O_4$; that is, whether the line CD would coincide with the line EF.

On Plate I. are also represented similar relations for ether, at volumes of 4000 and 1000 cub. centims. per gram ; the points representing the observed relations of temperature to pressure fall on the isochoric lines ; that is to say, the divergence from Boyle's and Gay-Lussac's laws is too small to be detected by experiment. At smaller volumes, however (those at 300 and 250 cub. centims. per gram are shown), the line passing through the observed points falls below the theoretical line, but is not quite parallel to it. It follows therefore, that if the isochoric lines are perfectly straight, they would cut the normal isochoric lines at an extremely high temperature. The physical meaning of this behaviour is that, if the temperature of a gas, at constant volume, be raised sufficiently high, the density must equal and then fall below the normal. It is evident that this must be the case. For the pressure of a gas depends on the number of molecules present in unit volume, on the average velocity of each molecule, and on the number of impacts on unit area of the surface of the containing vessel, in unit time. With constant volume, since the mean distance between the molecules remains constant, the cohesion of the molecules is assumed to be constant. But the rise of pressure produced by rise of temperature of a theoretical gas is based on the assumption that each impact takes place at the centre of each molecule; that is, that the actual volume of the molecules themselves is *nil*. But as this is not the case, as impacts must take place at some distance from the centres of the molecules, they must necessarily be more frequent. The effect of cohesion

is to reduce the pressure of the gas, by reducing the average velocity of the molecules, and this, for any given volume, by a constant amount. Hence, below a certain temperature, the pressure will be less than that of a normal gas, and if the temperature be reduced sufficiently, will become negative. With rise of temperature, the average velocity of each molecule will increase at the same rate as in the case of a theoretical gas; but the number of impacts, and, consequently, the pressure, will increase at a greater rate than if the gas were perfect; hence a temperature will ultimately be reached when the pressure is as much decreased by cohesion, as it is raised by the more frequent encounters of the molecules; and at that temperature the density of the gas will be normal. At still higher temperatures the pressure, and therefore the value of the expression $\frac{pt}{v}$, will be greater, and the vapour-density less, than that of a theoretical gas.

The formula which we find to represent the relations of temperature and pressure at constant volume, $p=vt-a$, represents these facts. The reciprocal of the vapour-density is the product of pressure and volume, divided by the product of the absolute temperature into half the molecular weight, or

$$\frac{1}{\text{vap. dens.}}=\frac{pv}{t}\times\frac{2}{\text{mol. wt.}}$$

Now while at small volumes the rate of diminution of volume of a substance increases more and more slowly with rise of pressure, there is no limit conceivable to the pressure which may be applied. Hence the value of the expression $\frac{pv}{t}$ must ultimately be greater than unity. The relation of $\frac{pv}{t}$ to the pressure for ether has been calculated by means of our equation for a few isothermals, and is shown in the annexed figure. From the direction of the lines, it would appear that if produced to still higher pressures, the product pv would reach and exceed unity. This state has indeed been reached by Natterer in compressing the so-called permanent gases at temperatures far above the critical points.

It is possible, by means of our equation, to follow these lines

into the unrealizable state, at low temperatures, where pressure becomes negative. An example is given at 150°. It is obvious that those isothermals which include negative pressures will intersect each other at the zero of pressure and $\frac{pv}{t}$, and will form loops in the negative region. This is shown on Pl. II.

It has recently been suggested by Wroblewski (*Wien. Monatsb. der Chemie*, 1886, p. 383), in a paper from the conclusions of which we differ in every point, that the minimum values of pv mapped against pressure form a curve continuous with the vapour-pressure curve. This is distinctly not the case. The curve is cut by the vapour-pressure curve at the critical point. It is, however, approximately continuous with the curve shown on plate ix. in our previous paper on this subject, representing the pressures corresponding to the inferior apices of the serpentine isothermals. The minimum product of pressure and volume probably does not occur at volumes corresponding to these apices, but at slightly lower volumes. We have proved this to be the case at high temperatures, and it is probably also the case at lower temperatures.

In conclusion, we should state that we have purposely omitted reference to the formulæ of Clausius, Van der Waals, and Sarrau. It appears to us that the relations of a and b in our formula to the volume is by no means so simple as that expressed by their formulæ.

VIII. *On the Production of Sudden Changes in the Torsion of a Wire by Change of Temperature.* *By* R. H. M. Bosanquet, *St. John's College, Oxford**.

[Plates IV. & V.]

The wire which forms the subject of the following observations is the finest platinum wire which can be procured; its diameter is about ·001 inch, and the length of the piece used was about 3 feet 6 inches. It originally formed the suspension of a galvanometer having a pair of nearly astatic needles about 7 inches long. The steel needles could be replaced by brass ones for the purpose of examining the properties of the suspension.

* Read May 14, 1887.

During the summer no disturbances worth attention were noticed; but in the winter, when the use of the stove occasioned rapid changes of temperature, disturbances became evident. The brass needles were employed throughout the present investigation.

The tables and diagrams which follow, except the last, all refer to one and the same state of the wire, the suspended weight not having been lifted during their progress. The lifting of the weight entirely changes the condition of the wire so far as these phenomena are concerned. The last table and diagram exhibit a subsequent state in which the phenomena are less distinct and in reversed direction.

The angular position of the needles was read on a scale of degrees attached to the circular glass shade surrounding them. This position, which we may call the elongation, is the abscissa of the diagrams. The temperature was measured by a very sensitive thermometer with a long spiral bulb, hung up alongside of the wire. The temperature in degrees Fahrenheit forms the ordinate of the diagrams.

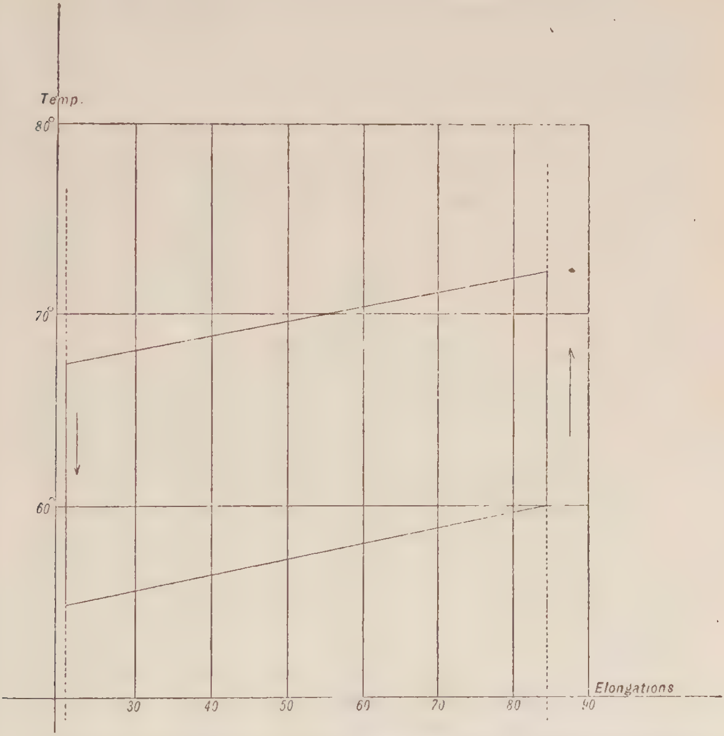
The principal effects are as follows:—The elongation varies between the limits of about 21° and 85°. Rising temperature drives it up to about 85°, where it remains while the temperature continues to rise. Falling temperature drives it down to about 21°, where it remains while the temperature continues to fall. The change of elongation is usually complete or nearly so by the time that the reversed change of temperature amounts to from 2° to 5°.

If the change of temperature is reversed as soon as the elongation reaches either of its limits, the line of the diagram returns along its original course: see diagram, April 27th. (The zigzags in the middle of this diagram arise from the rapid movement setting the needles in vibration.)

If the change of temperature is pushed in each direction after the limiting elongation is attained, we obtain a cycle of events which can be most simply represented by a parallelogram or series of parallelograms, such as the following:—

Here the vertical lines represent the limits of elongation, the one corresponding to rising temperature, the other to falling temperature. On reversal of the direction of change of temperature such lines as the sloping lines are described.

The inclination of these is nearly constant, and they may lie at any height between the lines of limiting elongation.



The above figure is a typical representation of the character of the principal effect; but the lines are never really straight, and there are subsidiary effects, which, however, in the present case are small compared with that above described.

The tables and figures at the end of this paper explain themselves.

There was some difficulty in obtaining that part of the curve which corresponds to the lower horizontal line of the typical parallelogram, as the corresponding change of temperature usually took place in the small hours of the morning. The diagram May 4th–5th shows a case in which this part of the curve was obtained. This exhibits a subsidiary effect, which consists of a tendency to increase the elongation * in

* *I. e.* to return from the lower limit towards the mean position.

the neighbourhood of the lower limit when the temperature is constant. Another very clear case of this effect will be found on the diagram for April 28th. My impression is that conditions of wire are common in which this effect is considerably developed. In such cases changes of temperature give rise to excursions, after which there is a more or less rapid return to a mean position.

The observations and diagrams hitherto referred to deal with changes in the angular position of a body hanging from the wire at rest. Observations were also made of the periods of vibration, for the purpose of ascertaining what changes took place in the torsional rigidity of the wire. The results of these observations are as follows :—

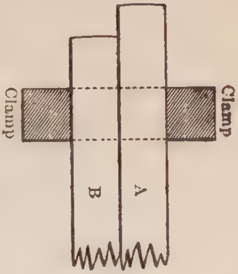
Periods of vibration.

Mean period.		Mean elongation.	Number of observed periods.
m.	s.		
4	45	89·2	4
4	53	88·5	4
4	41	85·4	13
5	0	84·6	4
5	39	83·7	4
6	3	77·0	4
5	52	72·1	12
6	33	57·9	4
4	23	54·6	2
5	33	46·4	4
5	16	37·0	2
3	56	35·0	8
3	4	32·3	2
2	29	28·6	2
2	40	27·5	4

It appears from these numbers that the period of vibration is least when the elongation is in the neighbourhood of the lower limit, increases rapidly as the limit is departed from up to a maximum value, and diminishes again slightly as the upper limit is approached and attained; that is to say, the torsion force has

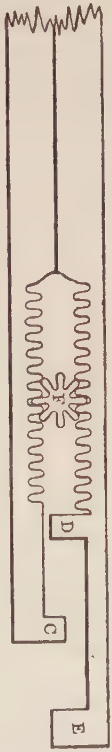
a maximum at the lower limit of elongation;
a minimum between the limits;
and an increase up to the upper limit.

The observations for the period are necessarily complicated by the changes in the position of rest, and they are not of great accuracy or consistency; but there can be no doubt that the course of the values is as above described.



It only remains to suggest what may be possible in the way of explanation of these phenomena. It appears to me that the only way of throwing any light on them is to imagine a mechanism capable of producing angular changes similar to those in question under the influence of changes of temperature.

A B are materials differently expansible by heat, capable of shifting, but with difficulty, through the clamp which binds them together.



Suppose C to be free between D and E; then any change of temperature will cause C to shift its position between D and E, and the pinion F will be turned round by the racks between which it lies. As soon as C comes up either against D or E the rotation of F is stopped, and the bars are compelled to shift in the clamp, if there is further change of temperature.

This arrangement exactly reproduces the typical changes observed in the present experiments. It is intended to serve the purpose of a formula.

The suggestion which we obtain from the above mechanism is that of loose contact between the parts involved, in this case probably between the molecules of the metal. As these peculiarities appear to originate in the extension of the wire by the suspended weight, it is possible that a partial separation of the molecules involving loose contact may arise out of this extension.

In any such case the molecules would bed against each other at the opposite parts of the loose contact, under the conditions which would give rise to either of the limiting elongations in the above experiments. The whole framework would be much stiffer when the loose contacts were thus

bedded together than when they were free. In this way it would be possible to account for the increased values of the torsion force at the limiting elongations, and its lesser values in the intermediate region.

Observations of Temperature and Elongation.

Time.	April 25th.		April 26th.		April 27th.		April 28th.		April 29th.		April 30th.	
	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elonga-
9 A.M.	46°·8	81·5	45°·7	77·0	46°·3	75·5	48°·8	75·0	50°·0	65·7	47°·2	81
9.15	47·8	84·0	46·0	77·0	47·5	79·2	49·5	77·0	50·4	71·3	49·0	82
9.30	48·0	78·0	46·7	78·5	48·1	80·2	50·5	80·0	51·0	75·0	49·8	82
9.45	48·2	79·0	47·2	78·5	49·2	81·0	51·7	80·5	51·5	77·5	50·3	81
10.0	48·7	78·4	49·0	79·0	50·9	81·0	55·0	80·5	51·7	78·3	54·0	82
10.15	49·0	79·0	48·7	79·2	54·0	81·0	60·6	80·0	52·1	79·5	58·0	83
10.30	49·8	78·3	50·3	79·8	57·3	81·8	63·8	83·5	53·8	80·5	62·0	84
10.45	50·5	78·0	49·5	79·2	60·2	81·2	67·5	84·0	58·4	79·5	66·2	81
11.0	51·0	78·3	49·7	78·8	62·5	81·3	70·6	81·6	63·5	78·3	71·5	82
11.15	51·4	78·3	49·5	79·2	63·3	82·2	72·4	81·2	68·3	80·2	74·1	80
11.30	51·6	78·3	49·6	79·2	65·2	82·5	73·3	81·2	69·6	80·5	76·2	81
11.45	51·6	78·8	49·7	79·4	66·5	83·0	73·5	81·5	70·0	81·5	76·2	81
12.0	51·8	78·8	50·2	79·5	68·9	83·0	72·8	81·5	69·8	81·5	77·0	82
12.15	52·4	79·0	50·3	79·7	69·4	82·3	72·4	82·5	69·6	81·5	76·3	82
12.30	53·8	78·5	...	...	69·0	82·3	72·0	82·0	69·5	82·5	75·0	82
12.45	54·0	78·7	50·2	79·3	69·2	82·5	71·6	82·0	69·0	82·0	65·0	78
1 P.M.	54·0	78·7	50·5	79·8	68·6	82·0	71·3	82·5	68·8	82·0	78·0	64
2.0	54·2	79·2	49·7	78·0	76·5	85·0	70·7	83·0	66·8	82·0		
2.15	54·2	79·2	49·7	78·3	77·0	83·5	70·4	83·0	66·2	82·2		
2.30	54·4	79·0	49·6	78·0	77·7	83·5	70·0	82·5	66·0	83·0		
2.45	54·4	79·0	49·8	78·5	78·7	84·5	69·2	82·5	63·2	76·0		
3.0	54·5	78·8	50·6	80·2	80·3	84·5	68·6	84·0	61·4	50·5		
3.15	54·5	78·8	51·4	82·0	82·4	84·5	68·4	83·0	60·7	37·0		
3.30	54·7	78·8	53·4	82·0	84·3	84·0	68·1	79·8	59·9	29·6		
3.45	55·0	78·5	57·6	82·3	84·4	84·0	67·6	83·5	59·6	29·5	59·3	32
4.0	55·0	77·7	60·3	83·0	84·6	84·0	67·0	77·5	59·0	27·5		
4.15	54·5	77·0	61·2	83·0	72·4	77·0	66·2	68·5	56·1	22·5		
4.30	54·5	76·7	62·0	84·0	70·0	65·0	65·5	61·0	55·5	22·5		
4.45	54·3	77·5	63·1	84·0	68·0	33·0	64·8	47·7	55·2	22·5		
5.0	54·3	77·5	64·2	83·3	66·6	26·3	64·3	40·0	54·6	22·0		
5.15	...	...	...	...	...	...	63·7	34·5				
5.30	55·3	85·0	64·0	83·2	64·0	22·5	63·3	30·4	53·6	23·0		
5.45	55·5	88·0	62·6	83·2	66·0	27·0	61·4	24·3	53·6	22·0		
6.0	55·2	82·0	62·6	82·3	68·5	36·7	60·1	22·0	54·8	22·5		
6.15	55·0	76·0	61·2	82·3	68·3	36·8	59·0	21·0	52·8	20·8	56·5	25
6.30	55·0	71·3	60·7	82·2	69·5	60·0	59·6	21·8	51·8	21·7		
6.45	54·4	60·5	60·1	82·4	70·7	70·0	59·1	21·3	51·3	21·5		
7.0	53·7	50·3	59·5	82·0	74·5	85·0	56·0	21·3	50·7	22·0		
7.15	53·0	40·3	59·0	80·0	76·1	85·0	54·8	21·0	50·0	20·8		
7.30	52·5	33·0	58·7	79·2	76·5	84·3	54·6	21·0	49·2	22·5		
7.45	52·2	29·5	58·1	78·5	76·0	83·5	54·6	21·5	49·0	21·0		
8.0	52·0	27·0	57·8	75·0	75·7	82·0	54·6	21·5	48·7	21·0		

May 2nd.		May 3rd.		May 4th.		May 5th.		May 7th.		May 9th.	
Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.
46·2°	75·5	51·7°	83·0	56·0°	36·5	58·0°	83·0	50·0°	122·5	57·4°	102·5
46·5	77·5	...	...	...	...	55·3	69·0	50·2	122·0	58·7	98·0
46·7	78·5	...	...	54·1	23·5	55·0	59·0	50·5	119·8	59·2	96·8
47·0	78·5	...	...	53·8	23·2	54·9	55·5	51·8	115·0	60·5	95·0
47·6	80·0	...	...	53·5	23·0	...	...	52·3	106·0	61·5	92·0
48·2	80·5	...	...	53·3	23·5	...	...	53·5	101·0	64·3	84·5
49·3	81·0	...	...	53·0	23·5	...	...	56·8	90·0	70·0	83·0
50·4	80·5	65·3	87·5	52·8	23·5	...	...	...	...	72·7	81·5
51·5	81·0	68·0	87·0	52·8	24·0	...	...	...	...	76·2	81·8
...	...	69·0	85·0	52·6	24·5	...	...	69·8	78·5	78·8	80·8
57·5	85·0	70·2	86·0	52·7	26·0	...	...	71·5	80·7	80·2	80·0
...	...	70·4	85·0	52·7	29·0	...	...	73·5	79·0	82·0	80·0
62·5	82·0	...	...	52·7	30·5	...	...	65·1	82·5	82·0	80·3
...	...	...	...	53·0	30·0	...	...	63·3	87·5	81·7	80·5
67·0	83·5	...	...	52·7	30·0	...	...	61·5	94·5	81·5	81·0
67·2	82·0	...	...	52·6	29·0	...	...	60·7	96·5	81·2	81·5
68·0	82·0	...	...	52·8	33·5	...	...	61·2	98·5	81·0	82·5
69·2	83·7	69·7	86·0	52·6	38·5	...	...	...	...	79·3	92·0
60·8	79·0	...	...	52·6	39·5	...	...	...	...	69·2	117·5
60·2	76·5	...	...	52·7	40·5	...	...	...	...	68·2	124·5
57·8	70·0	...	...	52·2	39·5	...	...	...	...	67·4	123·0
...	...	...	...	52·2	41·0	...	...	...	...	67·3	123·0
57·5	59·0	...	...	...	...	...	...	...	...	67·0	123·0
58·4	60·5	...	...	...	...	...	...	...	...	66·0	122·8
...	...	...	...	51·8	34·5	...	...	...	...	65·1	122·8
60·3	86·0	...	...	...	...	...	...	...	...	65·0	123·0
...	...	...	...	...	...	...	...	...	...	65·0	123·0
58·5	69·0	...	...	51·0	34·5	...	...	...	...	65·5	123·5
...	...	...	...	...	...	...	...	...	...	64·5	123·0
57·5	45·0	62·5	40·0	50·8	36·0	...	...	...	...	65·0	123·0
56·0	33·0	61·5	30·0	50·7	40·0	...	...	...	...	64·3	123·0
...	...	...	...	...	...	...	...	...	...	64·3	123·0
53·6	26·0	...	...	50·5	41·5	...	...	...	...	65·0	123·0
...	...	...	...	...	...	...	...	...	...	63·5	123·0
52·5	22·7	...	...	50·5	42·5	...	...	...	...	63·5	123·0
...	...	...	...	...	...	...	...	59·7	121·5	63·0	123·0
51·8	22·0	59·0	23·0	50·8	43·5	...	...	...	...	62·8	123·0

After May 5th the wire was readjusted, and the subsequent readings refer to an altered condition of the wire.

It would be easy to imagine several types of framework with loose contacts which would be capable of realizing the phenomena; but at present evidence in support of any particular arrangement would be wanting.

Supplementary Observations.

Date.	Time.	Temp.	Elongation.
April 25	9.30 P.M.	49°·0	24·5
„ 25	10.0	48·5	23·0
„ 26	10.45	50·8	22·5
„ 27	9.20	63·7	23·0
„ 28	10·20	54·5	25·0
„ 29	9.20	50·0	24·5
„ 29	11.0	48·6	25·5
„ 30	11.20	45·8	23·5
May 4	10.0	48·6	40·0
„ 4	11.0	56·8	86·0
„ 5	11.0	51·8	118·0
„ 6	7.45	51·0	124·0
„ 6	9.20	52·5	124·5
„ 8	9.15	59·0	124·0
„ 10	8.50 A.M.	54·7	143·5

IX. *Note on Beams fixed at the Ends.*

By W. E. AYRTON, *F.R.S., and* JOHN PERRY, *F.R.S.**

I. A HORIZONTAL beam with vertical loads, fixed at the ends, being given, to find the bending-moment everywhere: this is a problem the solution of which has not hitherto been put in an elementary form. A knowledge of the bending-moment everywhere leads, of course, to a complete knowledge of the strength and stiffness of the beam. The problem is quite soluble by a method which is obvious to any one who has worked out the theorem of three moments; but even for advanced students the work is tedious; and practically the answers known for the two cases—(1) when a uniform beam is loaded merely in the middle, (2) when a uniform beam is loaded uniformly—are regarded as roughly applicable in all cases which occur in practice.

If M is the bending-moment at a section, I the moment of inertia of the section about its neutral line, and E Young's modulus of elasticity for the material, then $\frac{M}{EI}$ is the curvature of the beam. If OO′ is a short length of the beam, $\frac{M}{EI}$ OO′

* Read June 11, 1887.

is the angle which the originally parallel sections at O and O′ now make with one another. Hence, if we divide the beam into a great number of parts, and if M is taken in the middle of each division, the moment of inertia there being I, the sum of all such terms as $\frac{M}{EI} \cdot OO'$ (if OO′ is one of the elementary lengths) gives the angle between the two end sections of the beam.

This principle, that $\Sigma \frac{M}{EI} \cdot OO'$ gives the angle between the two end sections of the beam, is, of course, well known. It is the basis of the theory of flat spiral springs. As applicable to beams, it may be put in the words of Professor Cotterill (‘Applied Mechanics,’ § 169, p. 334) :—“ . . . The angle (*i*) between two tangents to the deflection-curve of a beam is proportional to the area of the curve of bending moments [had he been speaking of non-uniform beams, Mr. Cotterill would have said the area of the curve whose ordinate is $\frac{M}{EI}$] intercepted between two ordinates at the points considered.” It is also the basis of Prof. Fuller’s beautifully simple method of dealing with arch-ribs fixed at the ends; and, indeed, it was when putting Prof. Fuller’s method before our students that we discovered the following simple application of the principle to beams. In spite of the apparent rashness of such a statement about such a well-ventilated subject as the elasticity of beams, we believe our method to be quite new.

II. In the case of a horizontal beam fixed at the ends, the angle between the end sections remains equal to nothing; and hence, in this case,

$$\Sigma \frac{M}{I} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Now, if the beam were merely supported at the ends, from a given system of loading it is easy to find what the bending-moment everywhere would be, either by numerical calculation or a link-polygon method. Let us suppose the diagram of this bending-moment m to be known. Then the bending-moment M of a beam fixed at the ends with the same loading, is $m-c$, where c is a constant; so that it is only necessary from the condition (1) to determine this constant c.

(1) is

$$\Sigma\frac{m-c}{I}=0, \quad \text{or} \quad \Sigma\frac{m}{I}=\Sigma\frac{c}{I},$$

$$\text{or} \quad c=\frac{\Sigma\frac{m}{I}}{\Sigma\frac{1}{I}}. \quad . \quad . \quad . \quad . \quad (2)$$

The rule then is, knowing m and I at every point:—Divide the beam into any number, n, of equal parts; find $\frac{m}{I}$ at the middle of each part, and add all the values together: this is the numerator in (2). Find $\frac{1}{I}$ at the middle of each part, and add the values together: this is the denominator in (2); so that c is known. Subtract c from every value of m, or diminish all the ordinates of the m diagram by the amount c, and we have the M diagram; that is, the real diagram of bending-moment of the beam fixed at the ends, with any distribution of loading and any variation in cross section.

III. If the cross section is the same everywhere, it is obvious that (2) becomes

$$c=\frac{1}{n}\Sigma m, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

if the beam was divided into n parts. In fact, c is the average value of m for the whole span. The application of (3) to beams of uniform section, fixed at the ends with one load in the middle or with a uniformly distributed load, is very simple.

IV. The condition (1) is applicable when I at every cross section of the beam is previously settled in any arbitrary manner. Let us give to I such a value that the beam shall be of uniform strength everywhere, that is that

$$\frac{M}{I}y=f_c \text{ or } f_t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where y is the greatest distance of any point in the section from the neutral line on the compression- or tension-side, and f_c and f_t are the constant maximum stresses in compression or tension to which the material is subjected in every section.

f_c may generally be taken as equal to f_t; and if d be the depth of the beam, y may generally be taken as equal to $\frac{1}{2}d$. Hence (4) becomes

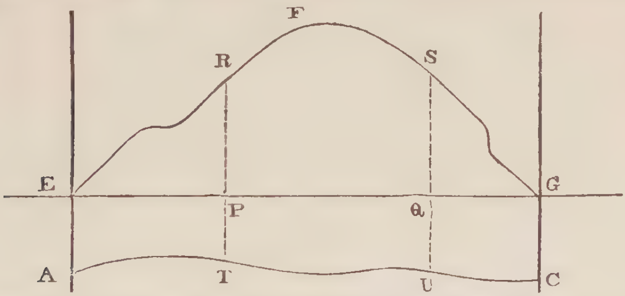
$$\frac{\mathrm{M}}{\mathrm{I}} d = \pm 2f, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

the + sign being taken when M is positive, and the − sign when M is negative.

Hence (1) becomes $$\Sigma \frac{1}{\pm d} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

the negative sign being taken from the ends of the beam to the points of inflexion, and the positive sign being taken between the two points of inflexion.

Now to satisfy (6) we have merely to solve the following



problem :—In the figure E A C G E is a diagram whose ordinates represent the value of $\frac{1}{d}$, or the reciprocal of the depth of the beam, which may be arbitrarily fixed; E F G E is a diagram of the values of m, easily drawn when the loading is known. We are required to find two points, P and Q, such that

$$\text{area ET} + \text{area QC} = \text{area PU}$$

and

$$\text{PR} = \text{QS}.$$

When found, these points P and Q are the *points of inflexion*, and PR or QS is what we called c. That is, $m - \text{PR}$ is the real bending-moment M at every place; and, knowing the bending-moment and depth d, it is easy to find I, as the condition of uniform strength is given in (5), or

$$\mathrm{I} = \pm \frac{\mathrm{M} \cdot l}{2f},$$

I being of course always positive.

We have found it very easy to solve this problem by trial. First find the area EC. Choose two points P', Q', whose ordinates $P'R'$ and $Q'S'$ are equal, and such that the area $P'U'$ seems to the eye nearly half the area EC. Measure the area $P'U'$, subtract from $\frac{1}{2}$EC, divide by about $4Q'U'$ or $4P'T'$ or by the mean value of these two. This will give an approximation to the error in the positions of P' and Q'. With an easy exercise of one's judgment, it will probably not be necessary to make a second approximation.

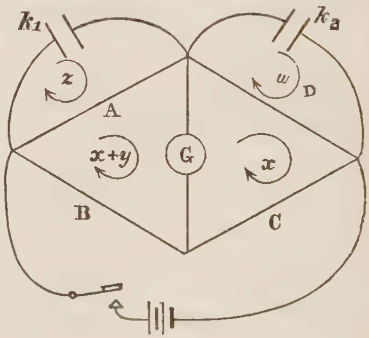
V. If the depth of the beam is constant, the problem of finding the diagram of bending-moment for a beam of uniform strength is of course solved by finding two points, P and Q, whose distance asunder is half the length EG of the beam, and at which the ordinates PR and QS are equal. Subtract PR or OS from every ordinate of the m diagram E F G, and we have the M diagram.

It will be seen that, with any system of loading and any variation of section in a beam, it is quite easy to get the diagram of bending-moment; and also that, with any system of loading and any variation of depth of beam, it is quite easy to get diagrams of bending-moment and moment of inertia for beams of uniform strength. Many other applications of the method are obvious.

X. *Note on Comparing Capacities.*

By E. C. RIMINGTON*.

LET two condensers, of capacities K_1 and K_2, be connected to the arms A and D of a Wheatstone-bridge, as shown in woodcut; and let



$$AC = BD,$$

so that there is a balance for permanent currents.

Let $x+y$, x, z, and w be the cyclic currents at some moment after the battery-circuit is broken, and when the condensers are discharging; and let q_1 be

* Read June 25, 1887.

the charge on K_1 and q_2 that on K_2 at this moment. Then we have

$$(A+B+G)(x+y)-Gx-Az=0*,$$
$$(C+D+G)x-G(x+y)-Dw=0;$$

also

$$z=\frac{dq_1}{dt} \quad \text{and} \quad w=\frac{dq_2}{dt}.$$

Therefore

$$(A+B)x+(A+B+G)y=A\frac{dq_1}{dt},$$

$$(C+D)x-Gy=D\frac{dq_2}{dt}.$$

Hence

$$y=\frac{\begin{vmatrix} A\dfrac{dq_1}{dt}, & A+B \\ D\dfrac{dq_2}{dt}, & C+D \end{vmatrix}}{\begin{vmatrix} A+B+G, & A+B \\ -G, & C+D \end{vmatrix}},$$

or

$$\Delta y = A(C+D)\frac{dq_1}{dt}-(A+B)D\frac{dq_2}{dt}, \quad . \quad . \quad (1)$$

where

$$\Delta = G(A+B)+(A+B+G)(C+D).$$

Integrating,

$$\Delta\int_0^\infty y\,dt = A(C+D)\int_{K_1Ax_0}^0 dq_1-(A+B)D\int_{K_2Dx_0}^0 dq_2,$$

where x_0 is the permanent current through A and D when the battery is on.

But $\int_0^\infty y\,dt$ is the quantity which passes through the galvanometer, $=q$.

Therefore

$$\Delta q = x_0\{K_2D^2(A+B)-K_1A^2(C+D)\}.$$

If x_1 is the permanent current in B and C,

$$x_0 = x_1\frac{C}{D};$$

* The self-induction of the galvanometer need not be taken into account, as the current in it commences and ends at zero. The arms A, B, C, and D are supposed to possess no self-induction.

and since AC=BD, we have

$$q = x_1 \mathrm{BD} \frac{\mathrm{K_2C - K_1B}}{\mathrm{G(B+C) + B(C+D)}}, \quad . \quad . \quad . \quad . \quad (2)$$

which may be written

$$q = x_1 \frac{\mathrm{K_2C - K_1B}}{\frac{\mathrm{G(B+C)}}{\mathrm{BD}} + \frac{\mathrm{C}}{\mathrm{D}} + 1}.$$

If now A and D be made infinite, which will obviate the necessity of first adjusting for a permanent balance,

$$q = x_1 (\mathrm{K_2C - K_1B}).$$

Also

$$x_1 = \frac{\mathrm{E}}{\rho + \mathrm{B} + \mathrm{C}},$$

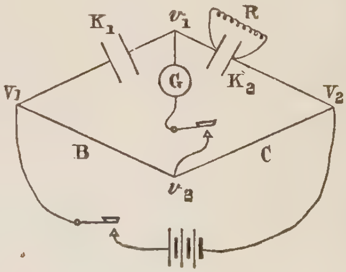
where ρ is the internal resistance and E the E.M.F. of the testing battery.

$$q = \frac{\mathrm{E}}{\rho + \mathrm{B} + \mathrm{C}} (\mathrm{K_2C - K_1B}).$$

If $q=0$,

$$\frac{\mathrm{K_1}}{\mathrm{K_2}} = \frac{\mathrm{C}}{\mathrm{B}}.$$

If a key be also put in the galvanometer-circuit, and the battery-key be first depressed and then, after a certain interval of time, the galvanometer-key, we have Gott's method of comparing capacities. This allows the condenser longer time to charge; and if the galvanometer when its key is depressed shows no throw, it indicates that the potentials v_1 and v_2 are equal, and that therefore



$$\frac{\mathrm{K_1}}{\mathrm{K_2}} = \frac{\mathrm{C}}{\mathrm{B}},$$

as before.

If, however, either of the condensers has an appreciable leakage, the result will be false by this method.

Suppose the condenser $\mathrm{K_2}$ has an insulation-resistance R, that of $\mathrm{K_1}$ being infinite. On depressing the battery-key, as

the condensers are in series, they take initial charges each equal to Q.

$$Q=(V_1-v_1)K_1=(v_1-V_2)K_2=(V_1-V_2)\frac{K_1K_2}{K_1+K_2}.$$

Let an interval of time t elapse before the galvanometer-key is depressed, and at the end of it let Q_1 and Q_2 be the charges on K_1 and K_2 respectively, and v the value of v_1. Then

$$Q_2=Qe^{-\frac{t}{K_2R}},\text{ where } e=2\cdot718,$$

$$Q_2=(V_1-V_2)\frac{K_1K_2}{K_1+K_2}e^{-\frac{t}{K_2R}}$$

and

$$Q_1=(V_1-v)K_1=\{(V_1-V_2)-(v-V_2)K_1$$

$$=\left\{(V_1-V_2)-\frac{Q_2}{K_2}\right\}K_1,$$

$$Q_2=(V_1-V_2)\left(1-\frac{K_1}{K_1+K_2}e^{-\frac{t}{K_2R}}\right)K_1.$$

Now

$$(v-V_2)=\frac{Q_2}{K_2}=(V_1-V_2)\frac{K_1}{K_1+K_2}e^{-\frac{t}{K_2R}}$$

and

$$(V_1-v)=\frac{Q_1}{K_1}=(V_1-V_2)\left(1-\frac{K_1}{K_1+K_2}e^{-\frac{t}{K_2R}}\right).$$

Hence

$$\frac{v-V_2}{V_1-v}=\frac{K_1e^{-\frac{t}{K_2R}}}{K_1(1-e^{-\frac{t}{K_2R}})+K_2}.$$

Also

$$\frac{v_2-V_2}{V_1-v_2}=\frac{C}{B}.$$

If, therefore, $v=v_2$, or there appears to be a balance,

$$\frac{C}{B}=\frac{K_1e^{-\frac{t}{K_2R}}}{K_1(1-e^{-\frac{t}{K_2R}})+K_2}$$

or

$$K_1=\frac{K_2C}{(B+C)e^{-\frac{t}{K_2R}}-C}.$$

So that, unless t be made very small compared to K_2R, we cannot compare K_1 and K_2 unless we know R and t. In the case of no leakage the conditions will be the same as if there were no key in the galvanometer circuit.

To find the best conditions for the test:—Taking equation (2), and substituting for x_1 its value

$$\frac{E}{\rho+\frac{(A+D)(B+C)}{A+D+B+C}}\times\frac{D}{C+D},$$

which equals $\dfrac{ED}{\rho(C+D)+D(B+C)}$, since $AC=BD$, we obtain

$$q=\frac{EBD^2(K_2C-K_1B)}{\{\rho(C+D)+D(B+C)\}\{G(B+C)+B(C+D)\}}.$$

If $q=0$, $K_2C=K_1B$.

Suppose a fractional error p to be made in the value of C. Then

$$q=\frac{EBD^2pK_2C}{\{\rho(C+D)+D(B+C)\}\{G(B+C)+B(C+D)\}}$$

$$=\frac{EpK_2}{\left(\frac{\rho}{D}+\frac{\rho}{C}+\frac{B+C}{C}\right)\left(\frac{G}{D}+\frac{GC}{BD}+\frac{C+D}{D}\right)}.$$

Now the throw is proportional to $q\sqrt{G}$ for a galvanometer whose coil-volume is constant. Therefore the throw is proportional to

$$\frac{EpK_2}{\left(\frac{\rho}{D}+\frac{\rho}{C}+\frac{B+C}{C}\right)\left(\frac{\sqrt{G}}{D}+\frac{C\sqrt{G}}{BD}+\frac{C+D}{D\sqrt{G}}\right)},$$

and this is to be a maximum; which it obviously is when

$$\sqrt{G}\,.\,\frac{B+C}{BD}=\frac{C+D}{D}\,\frac{1}{\sqrt{G}},\quad\text{or}\quad G=\frac{B(C+D)}{B+C}.$$

If A and D are infinite, the higher the resistance of the galvanometer the better. Also, in this case,

$$q=\frac{EpK_2}{\frac{\rho}{C}+\frac{B}{C}+1}=\frac{EpK_2}{\frac{\rho}{C}+\frac{K_2}{K_1}+1}.$$

Here q is largest when C is largest.

To find the conditions under which a telephone may replace the galvanometer :—

In equation (1) we have

$$\Delta y = A(C+D)\frac{dq_1}{dt} - (A+B)D\frac{dq_2}{dt}.$$

Now, in order to employ a telephone, y must always be zero. Therefore

$$\frac{A}{A+B}\,\frac{dq_1}{dt} = \frac{D}{C+D}\,\frac{dq_2}{dt}.$$

But

$$\frac{A}{A+B} = \frac{D}{C+D}, \text{ since } AC = BD.$$

Hence

$$\frac{dq_1}{dt} = \frac{dq_2}{dt}.$$

Now, since there is no current in the galvanometer, its resistance may be anything : let it be zero. Then K_1 discharges through $\frac{AB}{A+B}$, and K_2 through $\frac{CD}{A+D}$.

Consequently

$$q_1 = x_0 K_1 A e^{-\frac{t}{K_1\frac{AB}{A+B}}},$$

and

$$q_2 = x_0 K_2 D e^{-\frac{t}{K_2\frac{CD}{C+D}}}.$$

Hence

$$\frac{dq_1}{dt} = -x_0 K_1 A \,.\, \frac{A+B}{K_1 AB} e^{-\frac{t}{K_1\frac{AB}{A+B}}}$$

$$= -x_0 \frac{A+B}{B} e^{-\frac{t}{K_1\frac{AB}{A+B}}},$$

and

$$\frac{dq_2}{dt} = -x_0 \frac{C+D}{C} e^{-\frac{t}{K_2\frac{CD}{C+D}}}.$$

Therefore

$$\frac{A+B}{B} e^{-\frac{t}{K_1\frac{AB}{A+B}}} = \frac{C+D}{C} e^{-\frac{t}{K_2\frac{CD}{C+D}}},$$

and

$$\frac{A+B}{B} = \frac{C+D}{D}.$$

Therefore
$$K_1\frac{AB}{A+B}=K_2\frac{CD}{C+D}.$$
and, since $AC=BD$,
$$K_1B=K_2C,$$
$$\frac{K_1}{K_2}=\frac{C}{B}.$$

If there is self-induction in the four conductors, let L_1 be the coefficient of self-induction of A, L_2 of D, L_3 of B, and L_4 of C. Then
$$(A+B+G)y+(A+B)x+L_1\frac{d(x+y)}{dt}+L_3\frac{d(x+y)}{dt}-A\frac{dq_1}{dt}=0.$$
$$-Gy+(C+D)x+L_2\frac{dx}{dt}+L_4\frac{dx}{dt}-D\frac{dq_2}{dt}=0.$$
Solving for y, we have
$$\Delta y=(C+D)\left\{A\frac{dq_1}{dt}-L_1\frac{d(x+y)}{dt}-L_3\frac{d(x+y)}{dt}\right\}$$
$$-(A+B)\left\{D\frac{dq_2}{dt}-L_2\frac{dx}{dt}-L_4\frac{dx}{dt}\right\}.$$
Integrating,
$$\Delta q=\quad(C+D)\left\{A\int_{K_1Ax_0}^{0}dq_1-L_1\int_{x_0}^{0}d(x+y)-L_3\int_{-\frac{A}{B}x_0}^{0}d(x+y)\right\}$$
$$-(A+B)\left\{D\int_{K_2Dx_0}^{0}dq_2-L_2\int_{x_0}^{0}dx-L_4\int_{-\frac{A}{B}x_0}^{0}dx\right\}$$
$$=x_0\frac{C+D}{C^2}(BCD^2K_2-B^2D^2K_1+L_1C^2-L_3CD-L_2BC+L_4BD)$$
since $AC=BD$.

Also
$$x_0=x_1\frac{C}{D},$$
where x_1 is the permanent current in the branches B and C.
$$\therefore\ \Delta q=x_1.\frac{C+D}{CD}(BCD^2K_2-B^2D^2K_1+L_1C^2-L_3CD$$
$$-L_2BC+L_4BD).$$
If $q=0$,
$$BD^2(K_2C-K_1B)+C(L_1C-L_3D)+B(L_4D-L_2C)=0.$$
Hence
$$K_2C=K_1B,\quad L_1C=L_3D,\quad L_4D=L_2C;$$
or
$$\frac{K_1}{K_2}=\frac{C}{B}\quad\text{and}\quad\frac{L_1}{L_3}=\frac{L_2}{L_4}=\frac{D}{C}.$$

Again, since

$$\Delta=\frac{C+D}{C}\{B(C+D)+G(B+C)\},$$

$$q=x_1\frac{BCDK_2-B^2DK_1+L_1\frac{C^2}{D}-L_2\frac{BC}{D}-L_3C+L_4B}{B(C+D)+G(B+C)}$$

$$=x_1\frac{K_2BC-K_1B^2+L_1\frac{C^2}{D^2}-L_2\frac{BC}{D^2}-L_3\frac{C}{D}+L_4\frac{B}{D}}{\frac{BC}{D}+B+\frac{G(B+C)}{D}}.$$

If A and D are now made infinite,

$$q=x_1(K_2C-K_1B);$$

the same result that we obtain if there is no self-induction in the branches B and C.

XI. *The Effect of Change of Temperature in Twisting or Untwisting Wires which have suffered Permanent Torsion.* By HERBERT TOMLINSON, *B.A.**

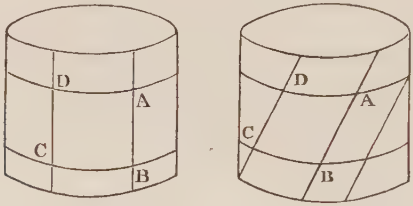
IN a paper recently communicated to the Physical Society†, Mr. R. H. M. Bosanquet has drawn attention to the remarkable behaviour of a very fine hard-drawn platinum wire, which twisted or untwisted very appreciably when subjected to small variations of temperature. As some experiments made by myself nearly eight years ago, but of which the results have not been as yet published, seem to bear on the interesting phenomenon in question, I now venture to bring them forward. I had previously been investigating the effects of permanent extension and compression on the longitudinal elasticity of wires in the following manner:—A wire of from 60 to 90 centimetres in length and 1 millimetre in diameter was suspended vertically, with its upper extremity clamped to a rigid support, and its lower one attached to a little brass block provided with a hook at the bottom, to which weights could be attached. Permanent torsion was imparted to the wire, which was afterwards subjected to various

* Read June 25, 1887.

† *Ante*, p. 50.

amounts of longitudinal stress. The permanent twisting of the wire in the direction of a right-handed screw causes the portion A B C D (fig. 1) to be permanently extended along the diagonal A C and compressed along the diagonal B D (fig. 2).

Fig. 1. Fig. 2.



If now the wire be loaded, the stress will produce temporary twist or untwist according as the elasticity is greater or less in the direction B D than in the direction A C. A full account of these experiments will be found in the 'Philosophical Transactions,'* so that it will suffice to state here that all the wires examined showed temporary twist or untwist to be the result of loading. The amount of twist or untwist produced by loading was comparatively small, and was measured by the usual mirror lamp-and-scale arrangement, the mirror being attached to the brass block at the end of the wire.

It then occurred to me that a similar arrangement might be used for testing the effect of permanent extension and compression on the thermal expansibility of a metal. A wire was suspended in the axis of an air-chamber, consisting of two concentric brass cylinders enclosing an annular space between them 6 millim. thick. The length of the air-chamber was 120 centim. and its outer diameter 10 centim. When the required amount of permanent torsion had been imparted to the wire, steam from a boiler was admitted into the annular space through a tube soldered into it near its lower extremity, and passed out through a similar tube near the top, where it was condensed in a vessel of cold water. After a period ranging from $\frac{1}{2}$ hour to 2 hours, when the *permanent* untwisting which invariably accompanied the heating had ceased

* Part I. 1883: "The Influence of Stress and Strain on the Physical Properties of Matter."

to become sensibly greater, the position of the light on the scale was noted and the action of the steam stopped. The annular space was now filled with cold water, and in about ten minutes afterwards the position of the light on the scale was again noted. The difference between the two readings was a measure of the amount of temporary twist or untwist produced by the change of temperature, the last being, on the average, 85° C. In several cases the heating and cooling were repeated with little or no variation in the amount of temporary twist or untwist. The length of each wire was nearly 120 centimetres and the diameter 1 millimetre.

The results of the experiments will be found in the following Table, in which are given the amount of temporary twist or untwist in scale-divisions, the amount of permanent untwist, and the load hanging on the wire during the experiment. The wires were all well annealed before being permanently twisted.

Metal ...	Copper.		Iron.		Aluminium		Silver.		Platinum.	
Load on the wire, in kilos.	1		1		½		1		1	
Number of complete turns of permanent torsion.	Permanent untwist produced by heating. P.	Temporary effect of heating. + signifies temporary twist, T.	P.	T.	P.	T.	P.	T.	P.	T.
10	......	.. ...	24	3·5−	800	74−	298	2·5−	...	4+
20	......	31+	...	......	...	71−				
30	......	......	...	6·0−						
40	......	......	...	......	...	......	...	3·0−		
50	555	36+	...	......	...	74−	...	......	63	10+
100	690	41+	...	5·0−	...	......	...	3·0−	138	
150	......	......	...	......	...	78−	...	......	273	12·5+
200	861	......	58	......	...	......	...	.....	...	13·0+

We may gather from the Table the following particulars:—

(1) Rise of temperature from 15° C. to 100° C. produces in permanently twisted iron, aluminium, and silver temporary

untwist, showing for these metals less thermal expansibility in the direction of permanent extension than in the direction of permanent compression. With copper and platinum, on the contrary, rise of temperature produces temporary twist.

(2) The amount of temporary twist or untwist is in all cases small compared with the amount of permanent untwist produced by rise of temperature. It is also absolutely very small*.

(3) A few turns of permanent torsion suffice to produce the maximum alteration of thermal expansibility which can be effected by this means.

Small variations of the load hanging on the wire produced no sensible effect on the temporary twist or untwist following on rise of temperature. The case, however, was different when large loads were employed. With platinum, for example, by increasing the load from 1 to 7 kilos. the temporary twist produced by rise of temperature was increased threefold. With copper also there was a similar effect produced by largely increasing the load. On the contrary, it would seem that with these metals, in which untwist is produced by rise of temperature, increase of load diminishes the untwist; and if the load be sufficiently large, the wire begins to be twisted temporarily by rise of temperature. This was found to be the case with annealed iron. The wire was twisted permanently with gradually increasing amounts of load on it, and after each twisting was slightly heated by running a burner up and down it. As before, with moderate loads there was temporary untwist on heating; when the load reached 10 kilos. there was neither temporary twist nor untwist; and finally, when the load was 16 kilos., there was very appreciable temporary twist.

Annealed piano-steel behaved like annealed iron; but with unannealed piano-steel rise of temperature caused a very appreciable temporary *twist*, instead of untwist, even with small loads on.

A remarkable feature about all these experiments was the great difference with different metals in the facility with which the effect of permanent torsion in one direction could be reversed by permanent torsion in the opposite direction. With copper, for example, one complete revolution in the opposite

* 100 scale-divisions only represent torsion through an angle of 2° 20′.

direction was sufficient to reverse the effect of 200 complete turns of permanent torsion, whilst with iron the difficulty of reversing the effect of previous torsion was considerable.

I am inclined to believe that the phenomenon observed by Mr. Bosanquet is to be, at any rate partly, attributed to unequal expansion in different directions. I have always found in wires which have been hard-drawn a certain amount, and sometimes a considerable amount of permanent torsion: this, we have seen, will cause temporary twist or untwist to be produced by rise of temperature. The amount of twist or untwist in any case, however, observed by myself was very much less than that observed by Mr. Bosanquet; and it would be of interest to ascertain how far the comparatively very large variation of torsion with small rise of temperature which occurred with the platinum wire used by him, is to be attributed to the comparatively great longitudinal stress on the wire*.

XII. *Remarkable Effect on raising Iron when under Temporary Stress or Permanent Strain to a Bright-red Heat.* By HERBERT TOMLINSON, *B.A.*†

It has been shown in the preceding paper that an annealed iron wire which has been permanently twisted is temporarily untwisted when the temperature is raised to 100° C., provided there is not too great a load on the end of the wire; but that if the load on the end of the wire is sufficiently great, a temporary twist follows on the rise of temperature. Fresh experiments were therefore entered on with the view of ascertaining whether, with a small load on the end of the wire, the temporary untwist produced by rise of temperature would be changed to twist when the temperature exceeded a certain limit. A few preliminary trials were made by merely heating a portion of the permanently twisted wire with a burner; and it was found that when the wire reached a bright red heat a most remarkable and sudden change occurred, the wire sharply *twisting* in the same direction as that in which it had previously suffered permanent torsion. When the burner was

* See above, the effect of increasing the load on the wire.

† Read June 25, 1887.

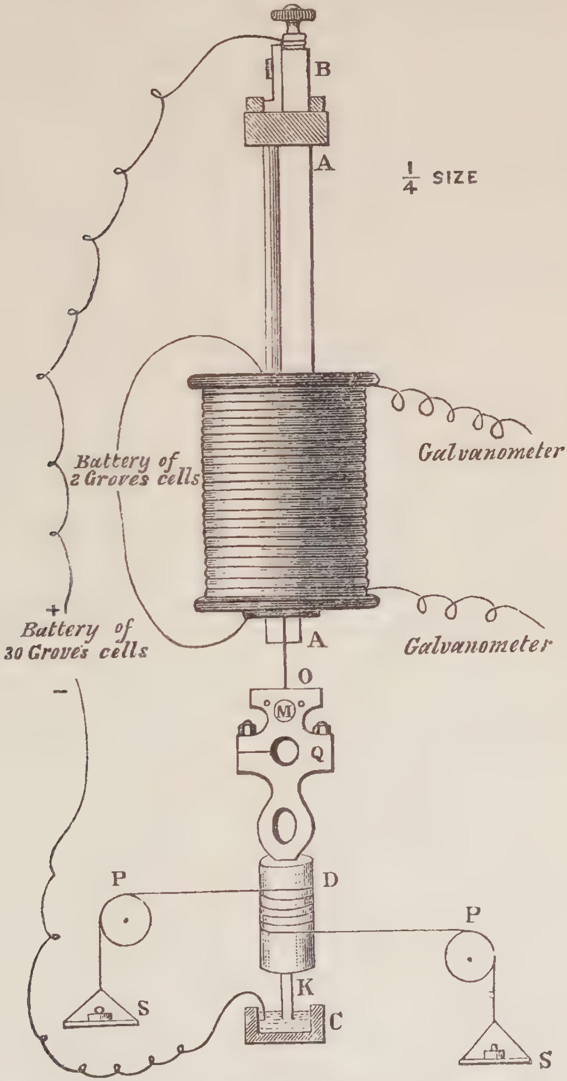
removed and the wire was cooled to a temperature which seemed slightly lower than that at which the phenomenon occurred in the first instance, it as suddenly untwisted ; and when it had again attained the temperature of the room, there remained a decided *permanent twist in addition to that which had been originally imparted**. This additional permanent twist is rendered more remarkable by the fact that rise of temperature beyond a dull red, but not extending to a bright red, is attended with a considerable *permanent untwist*. The phenomenon is evidently closely associated with one discovered by Mr. Gore so far back as 1869, and described in the 'Proceedings of the Royal Society' for that year. Mr. Gore's experiments were conducted in the following manner :—A thin iron wire, fixed at one end to a binding-screw, is attached at the other to an index which multiplies any motion of the wire ; the wire is stretched horizontally by a feeble spring, and is heated by an electric current or by a row of gas-jets. According to Mr. Gore, no anomalous action is observed on *heating* the wire to bright incandescence; but when cooling begins, the index moves back until a moderate red heat is attained, when suddenly the pointer gives a jerk, indicating a momentary *elongation* of the wire during the progress of its contraction. In 1873 Professor Barrett extended Mr. Gore's researches ; and in a paper full of interest †, entitled "Certain remarkable Molecular Changes occurring in Iron Wire at a low red Heat," showed that, under suitable conditions, the sudden change could be observed *not only on cooling, but on heating*. It is apparent, from the description given above, that the phenomenon observed by Mr. Gore resembles the one observed by myself, in that there is a sudden change in the iron when it has been raised to a bright red and afterwards cooled to a certain temperature. But in Mr. Gore's experiment the wire was under temporary *stress*, whilst in mine it was permanently *strained*; and whereas in the one case, on cooling below a certain temperature, there was a sudden *yielding* to the *stress*, in the other there was a sudden *diminution*

* This is not so if the wire be heated rather slowly ; but, on the contrary, in this case there is a very decided *permanent untwist* (see experiment V.).

† Phil. Mag. ser. 4, vol. xlvi. p. 472.

of permanent *strain*. A careful consideration of the results of my own experiments, and of those of Mr. Gore and Professor Barrett, satisfied me that in this, as in many other

Fig. 1.



instances, temporary stress and permanent strain act in opposite directions as regards their effects on the physical properties

of matter. I was moreover led to test, not only the effects of torsional stress and strain, but also those of other mechanical stresses and strains, for the most part with arrangements which will now be described.

The wire was suspended vertically in the axis of a glass tube, A (fig. 1), being clamped at its upper extremity into a brass block, B, resting on a wooden support fitting on to the top of the tube, and provided with a terminal for making connexion with one pole of a battery of thirty Grove's cells, arranged ten in series and three in parallel arc. The current from the battery passed through a set of resistance-coils arranged so that the resistance could be altered by small amounts at a time; the current was also conducted through an amperemeter and through the wire, passing in or out of the latter through the intermediation of a mercury-cup, C. The lower extremity of the wire was clamped into a second brass block, O, to which was secured a mirror, M, reflecting the light of a lamp on to a scale placed at a distance of one metre. This block was provided with a circular aperture, Q, into which, if necessary, a bar could be introduced, and at its lower extremity terminated in a brass cylinder, D, having a piece of rather stout brass wire, K, projecting from its centre, vertically downwards, and dipping into the mercury-cup C. When it was required to subject the wire to torsional stress, two fine silk threads were wrapped in opposite directions round D, and passed, as in the figure, over two fixed pulleys, P, to two small cardboard scale-pans, S, on which weights could be placed. When the wire was not required to be torsionally stressed the fine silk threads and the pulleys were dispensed with.

Experiment I.—A piece of very soft and carefully annealed iron wire*, 30 centim. long and 1 millim. in diameter, was subjected to torsional stress in the manner described above. The pans S each weighed 10 grms., and in each of them was placed a load of 20 grms., so that the torsional couple amounted to $30 \times 1{\cdot}6$ in gramme-centimetre-units†. A cur-

* This wire was specially prepared for me by Messrs. Johnson and Nephew, and is capable of suffering a permanent elongation of 25 per cent. before breaking.

† The diameter of D was 1·6 centimetre.

rent of gradually increased amount was passed through the wire, and when the temperature approached a bright red the wire began to twist rapidly and permanently under the influence of the stress*. The current was shortly afterwards stopped; and when a temperature between bright red and dull red had been reached the wire began suddenly to twist further, the amount of the sudden twist being about 90 degrees. When the wire had cooled down to the temperature of the room there was left a considerable permanent twist. In this experiment there was no perceptible temporary untwist on heating, but merely a slight check in the rate at which the wire was permanently twisting as soon as the critical temperature was reached. Other experiments were made with smaller and smaller torsional stresses until only the cardboard scale-pans were left to produce torsion: even here, however, it was impossible to get any sign of temporary untwist on heating. This mode of producing torsion was accordingly abandoned, and the stress was applied in the manner described in Experiment II.

Experiment II.—A cork was fitted into the central aperture of the block O, and through the centre of the cork was passed a knitting-needle about 25 centim. in length and $1\frac{1}{2}$ millim. in diameter, which was magnetized rather feebly by rubbing it with a small bar-magnet. The knitting-needle, when first placed in position, was nearly in the magnetic meridian, but afterwards the block B was turned until the needle eventually was nearly at right angles to the plane of the magnetic meridian, so that the wire might be under a feeble torsional stress due to the action of the earth's horizontal magnetic force on the needle. Immediately the wire reached a bright red temperature there was a very perceptible sudden temporary untwist, and on cooling a sudden temporary twist at nearly the same temperature†. This experiment was repeated with another piece of the same iron wire; but now the needle, instead of being twisted through 90 degrees from the north and south position, in which it originally lay when there was

* The *first* appearance, however, of sensible permanent twist occurred rather suddenly at *a dull red heat.*

† These effects can easily be shown by merely heating a small portion of the wire with a Bunsen-burner.

no torsion on the wire, was only twisted through about 10 degrees. Even the extremely feeble torsional stress now acting on the wire was not only sufficient to produce the phenomenon, but also a very decided *permanent* twist. The permanent twisting began directly the wire, on being heated, reached *a dull red heat*, and continued with increasing rapidity until the temporary untwist occurred at a bright red heat.

Experiment III.—The temperature at which, *on cooling*, the sudden change took place was evidently very much higher than that at which it occurred with the specimens of iron used by Mr. Gore and Professor Barrett; and this I felt inclined to attribute to the comparative softness of the iron used by myself. I accordingly tried several other specimens of iron and steel, both in the annealed and in the unannealed condition. With some of these the phenomenon did not occur on *cooling* until a dull red, or even at a still lower temperature; but it was found essential to its production that the iron *should have been previously raised to the temperature of bright incandescence**. On *heating*, the untwisting always showed itself at the temperature of bright red.

In consequence of the sudden change on cooling not occurring till a dull red heat had been reached in his experiments, Professor Barrett connected the phenomenon in question with another of equal interest. It is well known that, at a temperature of dull red, iron begins very rapidly to lose its magnetic properties; and, according to Professor Barrett, the two phenomena occur simultaneously. It is no doubt the case that, on *cooling*, the sudden jerk occurs simultaneously with the sudden regaining of magnetic properties in *some* specimens† of iron or steel; but it by no means follows that we have not two very distinct critical temperatures—one at or about a dull red, at which iron loses or regains its magnetic properties according as the wire is being heated or cooled; and another at a much higher temperature, namely near a bright red, at which sudden changes, certainly not less pro-

* This fact, which was also noticed by Mr. Gore and Prof. Barrett, is significant.

† The reason of this will be found in the remarks on the recalescence of iron.

found, occur. This is shown in the next experiment, which at the same time illustrates the fact already mentioned, that the effect of permanent strain is opposite to that of temporary stress.

Experiment IV.—The glass tube (fig. 1) was placed inside a magnetizing solenoid *, consisting of a single layer of cotton-covered copper wire $\frac{1}{20}$ inch diameter, and connected through a key with a battery of two Grove's cells. Surrounding the solenoid and concentric with it is a secondary coil, consisting of 840 turns of cotton-covered copper wire partly $\frac{1}{20}$ inch diameter and partly $\frac{1}{10}$ inch diameter. This coil has a resistance of about 1 ohm, and is connected through a key with a very delicate Thomson's reflecting-galvanometer of about 7 ohms' resistance. A piece of the same soft iron wire already mentioned was subjected to thirty complete revolutions of permanent torsion, and was finally released from all torsional stress. When the wire, on heating, reached the temperature of dull red, a momentary deflection of the needle of the Thomson's galvanometer took place, indicating a sudden loss of magnetic permeability; and as soon as a bright red heat had been attained it, as in the preliminary observations, *twisted* sharply and suddenly. The battery which was employed to heat the wire had its circuit now broken, and the wire was allowed to cool: first ensued a sudden momentary *untwist* of the wire, whilst *a few seconds afterwards* a kick of the galvanometer-needle in the opposite direction to that which had occurred on heating, indicated that the iron had suddenly regained its magnetic properties. This experiment was repeated several times; and though the sudden jerk on heating and cooling became less and less in intensity, and probably would eventually have vanished, it did not do so after six heatings and coolings †.

Experiment V.—The last experiment had shown the great persistency of the permanent torsional strain, even after the wire had been several times heated to a white heat. A fresh series of trials was made with iron which had suffered more

* Only the ends of this solenoid can be seen in the figure, the rest being enveloped by the secondary coil.

† I had the pleasure of repeating the experiment before the Physical Society.

or less permanent torsion, for the purpose of examining this persistency more closely. In these trials it was attempted to reanneal the wire after the permanent torsion had been imparted, by passing the flame of a Bunsen-burner very slowly up and down it so as to heat all parts in turn to a white heat, the wire being in some cases entirely free from any load on it, and in others having merely the slight load due to the block and its appendages. In no case could the sudden jerk at a bright red heat be got rid of entirely; though the same wire, when tested previously to imparting permanent torsion, had shown no trace whatever of the phenomenon. It is evident that iron cannot be satisfactorily annealed by the process mentioned above*.

These trials also brought out distinctly the fact that at a dull red heat the iron begins to *permanently* untwist rapidly, so that a considerable amount of the permanent twist originally imparted can be got rid of by maintaining the temperature for some time between bright red and dull red. Directly, however, the higher critical temperature is reached there is a sudden twist, which in the case of very soft iron is partly temporary and partly permanent, and in the case of steel wire which is unannealed, or hard iron wire, is principally permanent†.

Experiment VI.—A piece of the well-annealed iron wire, about 16 inches long, was clamped at one end and sustained in a horizontal position, save in so far as it was bent by its own weight. The wire was heated by a burner about three inches from the clamp. When the temperature reached a bright red, the wire began to bend permanently very rapidly by its own weight; and when the burner was removed and the wire cooled, a *sudden further bending took place* at a temperature a little below bright red; and when cooled to the temperature of the room, the wire remained permanently bent. Here the

* I write this because some observers seem to think that iron can be satisfactorily annealed in this way. In this, I believe, they are mistaken the iron should be maintained at a high temperature for a considerable period, and afterwards be allowed to cool very slowly.

† With some unannealed specimens I found it difficult to detect any trace whatever of *untwisting* temporarily when the iron *cooled* below the critical temperature.

attempt to unbend, which the wire no doubt made at the critical temperature when it was *heated*, was masked by the permanent bending. Another piece was therefore taken and heated at a point further away from the clamp, so that the bending-stress should not be so great. With care it was found possible to detect that the wire suddenly *straightened* itself when the critical temperature was reached on *heating*.

Similar trials were made with other specimens of iron and steel not so well annealed; and with these, when cooling, the sudden bending took place at a temperature lower than that at which it occurred with the very soft iron. With one specimen of wire, $1\frac{1}{2}$ millim. thick, it occurred at a temperature *apparently* below that of visible red*; but, as Professor Barrett justly remarks, the internal temperature of the thicker wires is no doubt masked by the cooling of the surface, whereas in thin wires the cooling throughout is extremely rapid. I found also that a piece of this specimen of iron *appeared* to lose its magnetic properties at a temperature below visible red, whereas with the thinner wires the *apparent* temperature at which this took place was somewhere about a dull red.

The very small amount of bending-*stress* which is required to bring out the phenomenon makes it a little difficult to detect with certainty the *opposite* effect of bending-*strain*; but it may be managed after a few trials in the following manner:—

Experiment VII.—A piece of the very soft iron wire was bent, as in fig. 2, with the portions A C, A B in a vertical position; the end C was secured to a clamp, and the bend A heated by a burner. If A B has been so arranged that its centre of mass is very nearly vertically above the part heated, there will be little bending-stress. In this experiment, on heating the bend A to a bright red, the end B jerked suddenly towards C, and when the burner was removed almost immediately jerked back again. If A B is arranged so that there is a little more bending-stress, it is curious to notice the struggle which sometimes ensues at a bright red heat as to

Fig. 2.

* Prof. Barrett also remarks that the phenomenon occurs on cooling at an *apparently* lower temperature with thick wires than thin ones.

which way the end B shall be jerked*. This experiment was repeated with other specimens of iron and steel and with similar results, except that on cooling, as with the torsional stress and strain, the phenomenon occurred at a lower temperature than with the very soft iron wire.

The question now arises, Is there any sudden *molecular* change at a bright red heat even where there is *no stress or strain affecting the wire?* Professor Barrett brings forward evidence in support of the view that there is, which I am afraid is not conclusive. He says:—"If, however, this molecular change be entirely due to alteration in cohesion, then the removal of the spring ought to cause the anomalous behaviour to disappear. But it does not. Without the spring, an iron wire can be seen by the naked eye to undergo a momentary contraction during heating, and a momentary and more palpable elongation during cooling†. Fixing one end of the wire, and bending the other extremity at right angles so that it may dip into a trough of mercury, and thus preserve contact with the battery, both actions can be seen." I have little doubt myself that the effects observed here were really due to the stress produced by the *weight of the wire itself.* So far as my own experiments go, I have not been able to detect any sudden change in the wire when sufficient care is taken to prevent the weight of the wire itself from producing an effect‡. Nevertheless I am inclined to believe, with Professor Barrett, that a sudden *molecular* change does occur at the critical temperature even when the wire is quite free from any mechanical stress or strain, though as yet experimental evidence is wanting to furnish sufficient evidence of such a change.

The Recalescence of Iron.

This curious phenomenon was, I believe, first discovered by Professor Barrett, and mention is made of it in his paper,

* That is, as to whether the effect of stress or that of strain shall predominate.

† The effect on cooling could always be got in my own experiments also with greater distinctness than the effect on heating.—H. T.

‡ It is difficult to realize, without actually putting the matter to the test of experiment, what a very small amount of stress or strain suffices to bring out the phenomenon.

already alluded to, in the following words :—" On September 12th I was examining the condition of the wire in a darkened room, when a new and unexpected change revealed itself. During the cooling of the wire it was found that just as it reached a very dull red heat, a sudden accession of temperature occurred, so that it glowed once more with a bright red heat. Illuminating the index and scale of the apparatus, which was watched by an assistant, it was at once found that the *reheating of the wire occurred simultaneously with the momentary elongation.*" Professor Barrett also ascertained that, in order to bring out the phenomenon, it was necessary *previously to heat the wire to a white heat* ; and, further, that *wherever the momentary expansion of the wire is feeble or absent, there likewise this recalescence is also feeble or absent.*

In my own mind I have little doubt that this beautiful phenomenon may be accounted for in the following manner:— When the iron has been heated above a bright red and is then cooling, it reaches the critical temperature at which there is a *tendency* for the sudden change to take place in the permanently strained or temporarily stressed wire : the change does not, however, *actually* take place at this temperature in consequence of the so-called coercitive force of the iron. When the iron is very soft and well annealed, the temperature at which the jerk takes place is comparatively near the temperature at which the jerk occurred on heating. But when the iron is hard-drawn or only imperfectly annealed, there may be a considerable difference in the two temperatures. Suppose, then, that the iron has cooled to the temperature at which its magnetic properties are suddenly restored. At this temperature a commotion more or less profound takes place among the molecules ; and this is sufficient to give them a start towards those positions which they have all along, after the higher critical temperature was passed, been trying to assume. When once started the molecular motion continues, the energy of position is rapidly converted into the energy of motion, and this again into the energy of heat, so that the wire once more glows. It by no means follows that the phenomenon of recalescence occurs with *every* specimen of iron at the critical temperature at which iron loses its magnetic properties ; indeed I have noticed sometimes *more than one* evident

sudden accession of heat* during the *same* cooling. Moreover, in the case of the very soft iron wire, with which the sudden jerk occurred at a much higher temperature on cooling than with the harder specimens, there was no perceptible trace of the phenomenon at a dull red heat. Probably there was a sudden check of the rate of cooling when the jerk occurred, though reglowing at this point was not noticed. I did not, however, pay much attention to the matter.

The view just advanced respecting the phenomenon of recalescence is, I think, justified by the following considerations :—

(1) The phenomenon is entirely absent in very well-annealed iron wire which has not, after annealing, suffered strain, nor is at the time under stress.

(2) It is not sensible unless the temperature of the wire has been *previously* raised to a bright red †.

(3) It becomes more and more sensible as the temperature at which the sudden jerk occurs in the wire becomes lower and lower, and is most pronounced in those wires in which the jerk does not take place until the wire suddenly loses its magnetic properties.

(4) It is not sensible unless there is a sudden jerk; the jerk and recalescence occur at the same instant.

Gore's phenomenon and the phenomenon of recalescence do not occur in pure, or nearly pure, specimens of nickel or cobalt; though, as is well known, nickel loses its magnetic properties at a much lower temperature than iron, namely from about 350° C. to 400° C. A most careful examination of three different specimens of nickel wire, procured from Messrs. Johnson and Matthey, was made‡. Unfortunately pure nickel wire cannot be drawn; but two out of the three specimens showed no trace whatever of the phenomena. The third specimen showed very obvious signs of both phenomena; but when tested was found, like iron, to lose its magnetic

* Sometimes there is merely a sudden check in the rate of cooling without any sensible recalescence.

† This is not the case as regards the phenomenon of iron suddenly losing its magnetic properties at a dull red heat.

‡ Professor Barrett had previously shown the phenomenon to be absent in nickel.

properties at a dull red heat instead of at the lower temperature at which pure nickel loses its magnetic properties, and therefore probably contained rather a large quantity of iron. The other two specimens lost their magnetic properties at a temperature of about 400° C. Only one specimen of cobalt was examined. Cobalt has not been drawn into wire as yet, though both Messrs. Johnson and Matthey, and Mr. W. Wiggin, jun., have kindly attempted to do so for me. Mr. Wiggin was, however, so good as to have rolled for me a specimen of cobalt, which is very nearly pure, in a strip about ½ millim. thick, 12 millim. broad, and 60 centim. long. This strip was tested with a Bunsen-burner and with a large blowpipe, and showed no trace at any temperature of either phenomenon when subjected to bending-stresses of various amounts. Unlike nickel and iron, cobalt does not lose its magnetic properties at any temperature at which it has been tested.

The Working of Iron at High Temperatures.

There can be little doubt that such experiments as these are important from a technical point of view. It is known, I am not aware whether generally or not, that it is dangerous to work iron within certain ranges of temperature. This one can easily understand from what has gone before; for it is manifest that if the temperature of a mass of iron is not the same throughout, and if the *mean* temperature of the mass be near the higher of the two critical temperatures, or if it is cooling from a bright red heat near either of the two critical temperatures, the effect of a blow or of any stress will be very different on different parts of the mass, and will be fatal to that uniformity of structure and strength which it is so desirable to procure.

Summary.

(1) There are two distinctly marked critical temperatures for iron; the lower somewhere about dull red, and the higher somewhere about bright red*.

(2) At the former of these critical temperatures the iron

* I hope at some future time to be able to fix these critical temperatures more exactly.

begins suddenly to lose its magnetic properties if it is being heated, and to gain them if it is being cooled.

(3) At or *near* * the latter critical temperature the iron, on being heated, if under the slightest torsional, longitudinal, or flexural stress or strain, begins to exhibit a remarkably sudden change. If the metal be under stress, the sudden change resembles that which would result from a sudden increase of elasticity. If the metal be permanently strained, there is a sudden increase in the amount of the strain.

(4) When the iron is cooling, a sudden change, but opposite in direction to that which occurs on heating, takes place. The temperature at which the sudden change takes place is, for very well annealed iron, nearly the same as that at which the change takes place on heating. If, however, the wire be not annealed, the change may be delayed by the so-called coercitive force of the metal until the lower critical temperature is reached. When the change is so delayed, the phenomenon of recalescence occurs.

* It is not unlikely that the so-called coercitive force may cause the change to take place at a slightly higher temperature than the critical temperature.